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THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND
CHEMISTRY;
AND
GUARDIAN OF EXPERIMENTAL SCIENCE.

CONDUCTED BY

WILLIAM STURGEON,

**SUPERINTENDENT OF THE ROYAL VICTORIA GALLERY OF
PRACTICAL SCIENCE, MANCHESTER;**

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THE ANNALS
OF
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JANUARY, 1842.

*Why do Electrified Bodies Recede from each other?**
By CHARLES GRIFFIN, Esq., M.L.E.S.

Read, in three parts, at the Electrical Society, Oct. 15th, Nov. 5th and 19th, 1839.

I shall in this, my third paper, (being the second on the above question,) attempt a somewhat more particular and definite explanation of the singular phenomenon of recession, according to the propositions at the end of my last paper.

I shall not pretend to go so far as to assert that this phenomenon must be explained entirely after my own manner, but endeavour to point out a mode that may perchance enable some electrician, versed in mathematics, to reconcile the doctrine of one electric fluid with the fact of the recession, not only of negatively, but also of positively electrified bodies, from each other, on principles long well known and established, without resorting to the mere assumption of a power of repulsion.

I have thought it best, considering my own ignorance of mathematics and the abstruseness of my subject, to err on the side of prolixity rather than on that of brevity; to risk saying something unnecessary, rather than leave anything necessary to my being understood, unsaid.

* The greater portion of my second paper, read to the London Electrical Society, (published in the "Annals of Electricity," vol. 3, p. 126,) was contained in papers transmitted to the Royal Society, on the 7th of June, 1833. In the same papers was stated generally my opinion that the electric fluid possessed the power of homogeneous attraction, as also in a letter in the "Mechanics' Magazine" of 3rd November, 1836, under the signature of "Corpusculum." The inverted commas in my said second paper in the Annals were intended to indicate the parts comprised in the papers transmitted to the Royal Society, but those commas are unfortunately misplaced. They comprised the greater part of such second paper, particularly the 1st, 4th, 5th, 8th, 9th, 11th, 13th, 15th, 16th, and first seven words of the 14th propositions. I may be excused this note, in order to anticipate charges of plagiarism.—C. G.

Ann. of Elec.—Vol. VIII.—No. 1.—Jan., 1842.

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(1.) Proposition 1.—I rest principally on the usual simplicity of natural operations, or paucity of causes employed in nature, to produce a multiplicity of effects.*

Prop. 2 and 3, I have before considered.

Prop. 4, 5, and 6, are, I believe, generally assented to.

Prop. 7 is not essential to the principal question discussed in this paper.

(2.) To illustrate prop. 8, let the inner circles, *a b c d*, plate 1, fig. 1, represent a portion of an indefinite line of metallic balls, equidistant, extending in each direction from the ball *b*, and supposed insulated, and the dotted circles their natural films or coatings of free electric fluid or liquid. The electricity of *b* is here attracted by the solid matter of its own ball, and also by the solid matter of *a* and *c*, and their solid matter is likewise attracted by the electricity of *b* and their other next adjacent balls, and the like of each ball throughout the series. Now, *a b c d* and the other balls being equidistant, and possessed of equal portions of free electricity, the electricity of either, as *b* for instance, cannot be attracted by the solid matter of its next adjacent balls, as *a* and *c*, in one direction more than another, the quantities of solid attracting matter in each direction being equal. Likewise, the solid matter of *b*, or any other ball, cannot be attracted in one direction more than another, the quantities of electricity attracting it in each direction being equal. Under these circumstances, the electricity of each ball may be termed *free electricity quiescent*.

(3.) Prop. 9.—To illustrate this proposition, let *b*, fig. 2, be one of the line of metallic balls before mentioned, having more than its natural share of free electricity, and *a c d* other such balls, having each its natural share only. Here, the free electricity of *b*, having been increased, must become active, and exert increased attractive influence on the solid matter of the adjacent balls, as *a* and *c*, whose free electricity has not been increased, and give those bodies an increased tendency to approach *b*. The free electricity of *b* may then be called *free electricity active*.

(4.) Prop. 10, fig. 2.—When the solid matter of *b*, which, in its

* When I say one fluid, I rather mean to deny that the positive and negative states are the separation of a vitreous from a resinous fluid, and not to assert that the common electric stream is composed but of one fluid. I think that that stream may be one principal fluid, (which I call electricity,) accompanied by or combined with caloric in a liquid state, or holding it, and perhaps very minute portions of metallic and other substances, in solution. It appears to me not improbable that the atmospheric atoms, as well as the surfaces of other bodies, are surrounded by films of liquid electricity and caloric, and perchance different strata of the two united in different definite atomic proportions; thereby giving rise to the varieties of colour, (as I endeavoured to shew some reasons for in the "Mechanics' Magazine" of 13th May, 1837,) and also causing that elasticity, or expansion and recession from each other, of the atmospheric atoms, which is so confidently attributed to a power of homogeneous repulsion, a power assumed (most paradoxically) to belong to the same two particles of matter, *when near together*, which are known and admitted to possess that of homogeneous attraction *when at a greater distance* from each other.

natural or unelectrified state, exerts a certain force of attraction on the *free electricity* of *c*, is required to exert an attractive influence on an additional quantity of free electricity communicated to itself, it must lose some portion of its attraction for the free electricity of *c*.

(5.) We might infer this by analogy, from the instance of a magnet; one, for instance, supporting a pound weight of iron, and capable of supporting that weight only. On the approach of another piece of iron, the magnet transfers a portion of its attractive influence to that approaching piece, and therefore drops the pound weight, and this although the aggregate forces exerted by the magnet on both pieces together, would be increased so as to equal more than a pound.

(6.) Were this otherwise, the attraction of two masses of matter (a piece of iron and a magnet, for instance,) would, to a considerable extent, increase in arithmetical proportion to the increase of one of them, and either of them support an enormous mass of the other.

(7.) Prop. 13, fig. 2.—[Referring to the words *vice versa*.] The natural quantity of *free electricity* of the ball *c* (adjacent to *b*), being required to exert a lessened amount of attraction in the direction *b*, is left at liberty to, and must, exert an increased attraction for the *solid matter* of the ball *d*. It will then have become *free electricity active*, as well as that of the ball *b*.

(8.) Prop. 14, fig. 2.—The *solid matter* of *c*, being called upon to exert an increased attraction by the added *electricity* of *b*, and thereby become active on that side of it, must lose some portion of its attraction for electricity in the opposite direction, (that of *d*, for instance,) and must have an increased *tendency* to approach the electricity of *b*, by the preponderance of the attraction in that direction.

(9.) As to the changes of attraction mentioned in the propositions 13 and 14, between a body (adjacent to an electrified body) and its own electricity, those propositions go too far; they certainly contradict each other on that point. However, as I do not see my way clearly, and as the settlement of the point does not seem essential to the main question of recession, I pass it by for the present.

(10.) Prop. 15.—This appears by proposition 14 and fig. 2. The *tendency* of *c* to part with electricity to *d*, and to abstract it from *b*, and of *d* to part with electricity to *e*, and abstract it from *c*, and the like of every more remote ball, is evident.

(11.) A portion of electricity will have been removed further from its own ball, and consequently have *diminished* its attraction for that ball, while it will have approached nearer to the next more remote ball, and thereby have increased its attraction for that ball.

(12.) When this attraction for *c*, or any more remote ball, shall have been so far increased as to exceed the force of attraction of *b*, or the next ball towards *b*, and the homogeneous attraction of the

electricity of the latter ball, a portion of electricity will dart in a spark to *c*, or other ball more remote from *b* than, but next to, the ball giving the spark. Under these circumstances, the free electricity of all the balls, on their sides farthest from *b*, as well as their *solid matter* on their sides nearest to *b*, may be termed *active*.

(13.) In further illustration of propositions 9, 10, 13, 14, 15, *vide* fig. 3, where *a* and *c1* each represent the commencement of an indefinite line of balls, and *c2* the commencement of two indefinite lines through *d2* and *d3*. Here the attraction for the electricity of an electrified ball, *b*, being divided between *a* only on the one side, and both *c1* and *c2* (a double quantity of next adjacent solid attracting matter) on the other side, the quantity of electricity accumulated on the *c* side of *b* must be much greater than on the *a* side.

(14.) This preponderance on the *c* side will also be further increased by *c2* being assisted in its inductive action by two lines of balls, *d2* and *d3*. For this latter reason, the quantity of electricity on the part of *b*, opposite to *c2*, (as at *f*,) will be greater than the quantity opposite *c1*, (as at *e*,) which is but the commencement of a single line of balls. Nevertheless, as the *c1* and *c2* lines are adjacent to each other, they will act inductively upon each other's electricity in some degree, and so far lessen the difference of their action on the electricity of *b*. Such mutual inductive action will, however, be small, being indirect or curvilinear.

(15.) The preponderant inductive influence exercised by *c2* on the electricity of *b*, through the assistance rendered it by the two lines of balls commencing with *d2* and *d3*, would be rendered still more preponderant by the assistance of another line or lines of balls concurring with them, or by those two lines themselves branching into three or more lines.

(16.) The converse of the foregoing explanations as to positive electricity, appears (if they be true) so evident in its applications to the negative state of electricity, that I shall not trouble the Society with a separate exposition of it.

(17.) The foregoing propositions are also as applicable to parts of bodies as to different bodies, and which parts may be considered as represented by the balls in the figures given; for, supposing those balls in contact, the only difference would be, that the balls which *tended* to part with their electricity, or discharged it by a spark only to others when separate, would, when in contact, part at once with it by conduction; and the same observation applies, if the balls in fig. 2, considered as in contact, be replaced by a cylinder; or in fig. 3, by a branching cylindrical body, each divided into imaginary sections as counterparts of the balls.

(18.) A *non-conducting* body, electrified on one point, would be in a situation more analogous to that of the balls when separate than when in contact.

(19.) In further illustration of propositions 9, 10, 13, 14, 15, *vide* also fig. 4, where $fghi$ indicate the vertical longitudinal section of a fixed and insulated metallic cylinder, the terminations being hemispheres of the same radius as the cylinder. This cylinder imagined to be surrounded indefinitely, in every direction, by small metallic balls, insulated, of equal size, and equidistant. It does not seem to require an argument to shew, that a large body electrified may act inductively on smaller ones. If the whole surface of the cylinder be (imagined so for the present), equally electrified in excess, a particle of free electricity at fgh or i , will have no direct and immediate tendency to change its situation on the cylinder, being in each case attracted, in every two opposite directions, by the solid matter of an equal number of the balls, and surrounded in every such two directions by *equal* portions of other free active electricity.

(20.) Not so as to any other particles. Take, first, those on the line gph only, (being one quarter of the longitudinal circumference of the cylinder,) for simplicity's sake, in the first instance, and compare those on the straight part, gp , with those on the curved part, ph , of that line.* While the plane, $lmgp$, continues of the same breadth at all distances from the straight line, gp , the plane, $mphoq$, increases continually in the length of its lines parallel to its curvilinear sides, in *arithmetical proportion* to distance from the curve ph ; so the number of balls through which that plane passes, and consequently its quantity of solid attracting matter, is greater, in each line across it, at *each greater distance*, than at any lesser distance from the cylinder, *in the like proportion*; while the number of balls, and consequently the quantity of solid attracting matter, in any line across the plane $lmgp$, parallel to the line gp , *do not become greater* at any distance, however remote. These conditions will tend to place *the first line of balls, adjacent and parallel to the line gp* , in a similar inductive state to ci in fig. 3; while *the first line of balls adjacent and parallel to ph* , will tend to acquire a similar inductive state to $c2$ in that fig. There will consequently be produced an unequal distribution of the electricity; a similar excess or preponderance of it on ph , as at f , in fig. 3; and a similar deficiency on gp , as at e , in fig. 3.

(21.) There will, at the same time, be an inductive action exerted upon the electricity of each other, by the solid matter of the balls in the two planes, about the line of their junction, mp , making their films of electricity to coincide there, and regularly increase in depth from g to h .

(22.) Take, secondly, the particles of electricity on the whole of the *surface* of the right-hand *half* of the cylinder, and consider the relations between the cylindrical part, gpi s, of that half, and the

* I take the liberty of frequently using the words line, plane, and section, as if they had thickness. The context will sufficiently indicate when they are used thus, or in a strictly mathematical sense only.

remainder or hemispherical part, $kphs$, and between their portions of electricity. In this case, the number of balls in each stratum, opposite and parallel to the surface of the cylindrical portion of the half cylinder, increase in *arithmetical proportion* to distance from that part of the surface; while the number of balls in the strata, opposite and parallel to the *hemispherical part*, increase in *geometrical proportion* to distance from the surface. This greater increase of matter by distance, opposite the hemispherical portion of the cylinder, enables each ball, in the first or nearest stratum, to exert a greater amount of attraction on the electricity of such hemispherical portion, than can be exerted by any ball in the first stratum opposite the cylindrical portion. The film of electrical liquid must, therefore, necessarily be deeper on the hemispherical portion of the surface $kphs$, and more shallow on the cylindrical portion $gpis$, in such proportion, that the excess in the quantity or depth of the film of electricity on the former portion, neutralises or balances the excess of attraction shewn to exist in that direction.

(23.) The balls in the cylindrical and hemispherical portions will nevertheless act upon each other's electricity, and render their films of electricity coincident in depth at the transverse section ps , and gradually increasing all the way from the transverse section or plane gi to h .

(24.) So far the electricity has been disturbed from the equable arrangement that would have been natural to it, (if attracted by the cylinder alone,) by the *opposing attraction* of matter exterior to the cylinder, and projects horizontally from the end of the cylinder towards the volume of air which attracts it, as a drop of water, hanging from the end of the finger by which it is attracted upwards, projects downwards by the opposing attraction of the earth. It now becomes requisite to consider what forces are exerted, in what directions, and with what tendencies, under these circumstances, between the unequally diffused electricity of the cylinder, and the solid matter of the *cylinder itself*.

(25.) The depth of the film of electricity being greater on any transverse section of the cylinder nearer to h , than on any other parallel section nearer to the section gci , (as on vv , compared with nn , for instance,) that greater proportional quantity or depth of film of the electricity over vv , and within the range of its influence, will require the solid matter of that section to exert a greater proportional amount of attraction than is required to be exerted by that of the section nn , on the smaller depth of electricity over it, and within the range of its influence.

(26.) The solid matter at section nn , will therefore be at liberty to exert a *greater proportional amount* of attraction or inductive influence, than the solid matter at section vv , on a portion of electricity equidistant from them, as is the electricity round the section pks . The tendency of this greater amount of attraction

will therefore be to move the electricity, situate round pks , towards nn , and would do so, were it not for the opposing attractive force of the air, as above explained.

(27.) Action and reaction being equal, this greater amount of attraction also *tends* to draw the solid matter at section nn towards the electricity round pks , and, as a consequence of the union of section nn with the rest of the cylinder, to move the whole of the cylinder in the line or direction towards o .

(28.) Causes exactly similar, and equal to those considered as operating on one-half the cylinder, operating also on the other half, the *tendency* of the cylinder to move towards o is counteracted and balanced by an equal and contrary *tendency* of such other half, in the *opposite* direction.

(29.) The two halves of the cylinder, being separated through the transverse plane lcr , and suspended by silk threads, will then no longer oppose and restrain each other's motion; but obeying their *separate and opposite tendencies*, RECEDE from each other. Thus might take place a perfect recession of two electrised bodies from each other, without reference to a power so anomalous as is that of *repulsion*, particularly when referred to the negative state of electricity.

(30.) As the right half of the cylinder moves towards o , the balls included between the stationary medial plane lcr , and the parallel plane $mk t$, considered as moving with the cylinder, become increased in number, and less and less influenced by the inductive action of the electricity of the other, now more distant, half of the cylinder; while the quantity of solid attracting matter exterior to the cylinder, on the side vv of the plane $mk t$, is not increased.

(31.) Several consequences flow from this increase in the number of balls or quantity of solid matter between the medial stationary plane lcr , and the moving plane $mk t$, and each of such consequences flows from the other or others before it, in the following order:—

(a) An increase in the attraction or inductive influence exerted on the electricity of the first stratum of balls round nn , by the solid matter of the increased number of more remote balls.

(b) An increase in the attraction of the solid matter of such first stratum of balls, for the electricity of the cylinder about nn .

(c) An increase in the depth of the film of electricity about nn , at the expense of the mass of it at vv , and elsewhere on the side of $mk t$, towards h .

(d) A diminution of the excess in the depth of the electrical film at vv , over that at nn .

(e) A diminution of the excess of attraction exerted between the solid matter of section vv , of the cylinder and its electricity, over that exerted between section nn and its electricity.

(f) A diminution of the excess of the attraction of such solid matter at nn , for the electricity round the plane or section pks , over that of the solid matter at vv , for the same electricity.

(g) A diminution of the tendency of that electricity, and the solid matter of the section nn , to approach each other.

(h) And, lastly, a diminution of the tendency of this half of the cylinder to recede from the other.

(32.) In lieu of the two half cylinders, I will now examine the case of two balls, equally electrified positively, $b\ 1$ and $b\ 2$, fig. 5, which (the foregoing explanations being admitted) must divide the inductive attractions of the balls a and c between them. Here $b\ 1$ will be in the situation of an adjacent ball to $b\ 2$, as also $b\ 2$ to $b\ 1$, and the observations on propositions 9 (3), 10 (4, 5, 6), 13 (7), 14 (8), 15 (10), and fig. 2 (11, 12), where c was a body adjacent to b , apply in the same manner to shew that the free and increased electricity of $b\ 1$, fig. 5, must now stand out or be elongated on its side remote from $b\ 2$, as the free and natural portion of a and c , fig. 2, stood out on the sides remote from b ; $b\ 1$ and $b\ 2$, fig. 5, being equally electrified, any observation applying to one must apply equally to the other.

(33.) The two balls, $b\ 1$ and $b\ 2$, if kept in contact, act very nearly as if they were one elongated body, like the above-mentioned cylinder, fig. 4; and well known experiments having proved that such a body has a deeper bed, or accumulation of electricity, at its ends than elsewhere, support the foregoing partly theoretical conclusions. Let the two halves of the cylinder in fig. 4, and the balls $b\ 1$ and $b\ 2$, in fig. 5, change places, and it will be seen that the above observations will, with a trifling allowance, still apply to each figure under such a change of circumstances.

(34.) I have thus far spoken of metallic balls for the sake of more easy comprehension, (without noticing the influence of the air,) but think the observations hitherto made will apply equally to the atoms of the atmosphere, which are, by some yet unexplained means, insulated from each other. That they, in their natural state, possess *free electricity*, is shewn by a point on a conductor electrified negatively, and I suppose it adheres to them as it does to a metallic ball. If they be chemically compounded, and consequently not spherical (speaking of their component atoms of oxygen and nitrogen only), those component atoms probably present spherical or otherwise curved terminations, and answer every purpose of the present question. I shall therefore take the circles in the figures given as their representatives, and consider either a metallic ball, or one of such atmospheric atoms, positively electrified, will produce the same inductive influence, or disturbance of equilibrium, amongst other atmospheric atoms, as one electrified metallic ball amongst others unelectrified, and consequently the same elongations of their films of free electrical liquid, in a direction from the positively electrified ball or atom, towards each next adjacent atom.

(35.) Returning to the case of two electrified balls. Let two such balls as b and c , fig. 6, be positively electrified, suspended as the common pith ball electroscope, and surrounded as usual by

atmospheric atoms, of which a few only are represented by the circles *a d e*, 1, 2, 3, &c. Let the lines *rs*, *tu*, *vx*, *yz*, *mn*, represent vertical planes perpendicular to an imaginary straight line, passing through the centres of *b* and *c*, and each adjacent pair inclosing equal spaces. Let the two outer of those spaces be called respectively the outer *b* and *c* spaces, and the others the inner *b* and *c* spaces. Let also the imaginary planes, *tu* and *yz*, passing through the centres of *b* and *c*, be imagined also to recede when, and as those balls recede; and the planes *rs*, *mn*, to do the like, but twice as rapidly; so as to preserve the same equality of distance between every two adjacent planes, at each instant during the recession of the balls.

(36.) Let the electricity be—*First*, (*supposed* for the sake of argument,) *equally diffused* over each ball, even when the balls are brought near each other.

Secondly, distributed *unequally* by inductive action; and

Thirdly, causing the balls to recede from each other.

(37.) *First*, The electricity *supposed* equally diffused over each ball, as I suppose it would be, if left to the undisturbed action of the balls alone, and under this head—

As to atom *e*, midway between *b* and *c*. The solid matter of *b* and *c*, being required to exert increased attraction by an additional quantity of free electricity upon themselves, loses a portion of its natural attraction for the free natural electricity of *e* (prop. 10). That electricity is therefore left at liberty to exert increased attraction for the common matter of all the circle of next adjacent atmospheric atoms (which includes atom *i*), in the plane *vx*, and other atoms next adjacent to *e*, near that plane, (proposition 13,) under the words *vice versa*.

The electricity of *e* will thereby be partly accumulated in a circle round it, in and near the medial plane *vx*, and evince a tendency to escape to atom *i*, and the other atoms similarly situated (prop. 15.) This atom, *e*, will thus be attracted *by means of its electricity*, by equal forces towards every atom next adjacent to it in the plane *vx*, and by other equal forces *by means of its common matter* towards *b* and *c*.

(38.) The electricity of atom 2, and the other atmospheric atoms, whose common matter is thus acted upon by atom *e*, will at the same time act on the next larger circle of atoms, including atom 2, and those adjacent to such circle last mentioned, (prop. 14 and 13.)

(39.) As to atom *d*. The *common matter* of this atom corresponds to that of *c*, in figs. 2 and 5, and is subject to the same observations, and therefore is attracted by the electricity of *c*, fig. 6, with great force, in that direction; while its *own electricity* is attracted with great force in the opposite direction, or away from *c*, by the other atoms in that direction, which are acted upon in a similar manner by the next in succession, and so forth. The same observations apply to atom *a*, with respect to the ball *b*.

(40.) As to atoms 1 and 2. These atoms, and all other situate

in the plane vx , equidistant from b and c , form the boundary dividing the respective ranges of predominant inductive action exerted by b and c ; both 1 and 2, and their electricity, being as equally subject to the separate inductive action of b and its electricity, as to that of c and its electricity.

(41.) As to atom 3. The electricity of atom 3, and all others situate in the plane, yz , must lean a little to the right of yz , as represented, there being a preponderance of atmospheric atoms on that side, capable of being acted upon, and reacting, by induction.

(42.) This preponderance may perhaps appear more distinctly, by observing that the outer c space is equal to the inner c space; but the action of the electricity of c , on the atoms in its inner space, being counteracted by the inductive action of the electricity of b , while there is little or no such counteraction in the outer c space, the action of the atoms in the latter space may be certainly concluded to exceed the action of those in the inner space. The whole of the atoms to the right of mn may then be considered as adding to this excess, by exerting a great preponderating force, both in direct and oblique directions, *only from c* , without any counterbalancing force tending *towards c* .

(43.) Were there such a counter force, it could only be found to the left of vx ; but, that such a compensating force cannot be found there, appears certain; for the electricity of b must act on the common matter of the atoms there, and neutralise, or at least balance, by their propinquity, all the effects, either immediate or inductive, of that common matter, on the electricity of c . The preponderating force, to the right of mn , will consequently incline the deepest part of the film of electricity of atom 3 to the right of yz .

(44.) So far as to the electricity of b and c , considered as *equally diffused*.—Now,

Secondly, As to a new distribution by induction.—Before this new distribution, the electricity on the two contiguous hemispheres of the balls, acting conjointly on all the atmospheric atoms in the plane vx , while the atoms in the plane mn were acted on (at least, near the balls) by the outer hemisphere of c only, a greater amount of induction, or disturbance of equilibrium, must take place on the atoms in the plane vx , than on those in the plane mn ; and consequently, give the former the stronger tendency to part with their electricity.

(45.) Though there be this joint action of the electricity of the two inner hemispheres of the balls on the atoms in the plane vx , yet a *moiety* of this action cannot equal the action capable of being exerted by the electricity of *one* of such inner hemispheres, if acting *alone* and unopposed; and, consequently, such *moiety* will not equal the action exerted between the electricity of the outer hemisphere of c and the atoms in the plane mn ; and, in the same manner, a *moiety* of the actions of the electricity of the two inner

spaces, will not equal the action of the electricity of the outer hemisphere of *c*, on the atoms in the outer *c* space.

(46.) Therefore, on the electricity of *b* and *c* being released from the unnatural assumed distribution, and the natural forces allowed to operate, the preponderance of inductive force in the outer *c* space, must draw a greater portion than half of the electricity of *c* to its outer hemisphere; and when, to this drawing force, we add the totally unbalanced attractive force of the atoms to the right of *m n*, very little electricity can be left on the inner hemisphere of *c*. All the foregoing observations, as to the ball *c*, &c., apply, of course, equally to the ball *b*, &c.

(47.) *Thirdly*, As to the ultimate effect, the recession of the balls from each other.—The said preponderant force, existing on the right of *m n*, acting, by its inductive influence, through the atoms in the outer *c* space, and drawing the electricity of *c* in that direction, and the continual attraction of the electricity of *c* for the solid matter of *c* itself operating as shewn by fig. 4, (25, 6, 7,) *tending* to draw that solid matter into the centre of its film of electricity, while exactly similar forces are exerted on the ball *b*, and its electricity in the opposite direction, the recession of *b* and *c* from each other necessarily results.

(48.) As the ball *c*, and, consequently, the plane *y z*, recede from the stationary medial plane *v x*, an increased number of atmospheric atoms must become included between those two planes, that is, in the inner *c* space, and require a doubly rapid removal of the imaginary plane, *m n*, to include an equal additional number of atmospheric atoms in the outer *c* space, to balance the increased numbers in the increased inner *c* space. This process lessens the preponderant force to the right of *m n*, or beyond the outer *c* space, by increasing the distance of the atoms which, for the time, exert that force, and will so continue to lessen that preponderant force, till it is so far reduced as to be balanced by the gravity of the ball and the action of the thread supporting it.

(49.) To assist in such process, there is of course, at the same time, a *progressive return* of electricity to the contiguous sides of the balls, in proportion to the increased number of atoms, capable of induction, which have become included in their inner *b* and *c* spaces, as explained with respect to the receding halves of the cylinder, fig. 4, (30, 31.)

(50.) It may not be entirely useless, nor uninteresting, to inquire how far the balls might possibly recede from each other, supposing the supporting thread removed, the balls in an atmosphere of indefinite extent in every direction, of equal density, and perfect mobility.

(51.) If the law (which, I believe, is generally considered as settled), that electrical attraction, and what is called repulsion, are inversely as the square of the distance, be true, then a single electrified ball, placed in the supposed indefinite atmosphere, must

hemispheres on the atmospheric atoms in the two inner *b* and *c* extend its influence or disturbance of equilibrium indefinitely in every direction. Let the inner circle, fig. 7, represent a section of such ball, and the spaces between each two adjacent rings, the sections of spherical strata of atmospheric atoms, with their centres coinciding with the centre of the ball.

(52.) The sum of the attractions of the *free active electricity* of the atoms in any one spherical stratum, for the *common matter* of those in the next more remote stratum would (if the law be as above stated) equal that between the *electricity* of the ball and the *common matter* of the first stratum; and, consequently, the sum of such attractions between any one pair of next adjacent strata, (strata 2 and 3, for instance,) would exactly equal the sum of those between any other pair of strata next adjacent to each other, however distant those pairs of strata might be from the ball, or one pair of strata from the other pair.

(53.) Thus calling the number of atoms in the first stratum = *g*, and the sum of their attractions for the electricity of the ball = 1, the forces would stand thus:—

Strata.	Number of atoms in each stratum.	Force in each stratum of <i>g</i> of atoms.	No.	Aggregate of forces in each stratum.
1st....	<i>g</i>	× 1	=	1
2nd....	4 <i>g</i>	× 1-4	=	1
3rd....	9 <i>g</i>	× 1-9	=	1
4th....	16 <i>g</i>	× 1-16	=	1
12th....	144 <i>g</i>	× 1-144	=	1

And so forth, for any number of strata whatever.

(54.) But I suppose the attractions exerted by the *electricity* of the ball on the *common matter* of the atoms of the first stratum is (contrary to the supposed law) lessened, or partly neutralised, at that and every subsequent stratum, *by the attraction between each atom and its own electricity*, such attraction tending to an equal diffusion of that electricity over every part of its surface.

(55.) This continual deduction from the original force, I presume, would, at some finite distance, entirely overcome it, and that force would, in that case, be divided equably, though not equally, amongst all the atoms, in a limited spherical space, having the electrified ball for its centre. The ball would thus be opposed by all the surrounding atoms, in a struggle, as it were, for the possession of the electricity added to it. The force of *the ball* would be exerted on such electricity in a direction *towards* the ball's centre, and the forces of all the *atmospheric atoms* exerted on the same electricity, would be *from* the centre of the ball; but the forces of each stratum after the first, would not be immediate, but be exerted, as before shewn, through the medium or inductive agency of all the strata nearer to the ball; as the lower links of suspended chains do

not act immediately on the higher, but only by communication through those intermediate.

(56.) Admitting the law and the imaginary conditions above mentioned, and applying them to the balls *b* and *c*, fig. 6, the before mentioned preponderant force to the right of *mn*, or beyond the outer *c* space, would never be wholly destroyed, however far the balls might recede from each other. Many of the spherical strata of atmospheric atoms, within the influence of each ball, must impinge on and intersect similar spherical strata round the other ball at the plane *vx*, and prevent the inductive influence of either ball being exerted with the same effect in the portions of its spherical strata, which shall be cut off from the larger portion by the plane *vx*, and of course prevent the inductive influence of each from being exerted with the same force, on its side nearer to the other ball, as on its remoter side.

(57.) But *excluding the law*, and even admitting the other imaginary conditions (prop. 50), then the balls will recede only till the inductive force of the electricity of each ball, exerted in every direction, is equalled and balanced exactly by the resisting attractions of the *common matter* of such balls, and the surrounding atmospheric atoms, for their own several electricities, and perhaps by other retarding or opposing causes.

(58.) Then, also, two perfect spheres of disturbed equilibrium, having the two balls for their centres, will meet at the atom *e*, without being able to cause its electricity to recede towards the next adjacent atoms, 1, 2, &c., in the plane *vx*, so as to act upon them by induction.—*Vide* note at the end.

(59.) The dissipation of electricity, whether positive or negative, from points to atmospheric atoms, is an instance of recession apparently explainable in the same manner, as the recession of one electrified ball from another.

By fig. 3, it is shown how one ball (or atom), as *c 2*, in its natural state, becomes, by its neighbourhood to an electrified ball, more changed in its electrical condition than another, as *c 1*; that is to say, by its induction being assisted by, or transmitted to a greater mass of matter, on its side remote from the electrified ball, as by or to *d 2* and *d 3*, and the two lines of balls through which they are supposed to transmit their inductive influence.

(60.) Fig. 4 (19, et. seq.) furnishes a further illustration of this; and the ends of the cylinder are similar in principle to points, especially when we consider such cylinder as becoming more elongated than in the figure, and also reduced in thickness; the whole cylindrical surface approaching the axis *fk*, and the surface of the right hemispherical termination contracting equally towards its central point *k*, till the radius of both cylinder and hemisphere are reduced to (say) one-fourth of their original length.

(61.) By this change, the number of balls forming the first stratum round the cylindrical part of the cylinder, (supposing them

moveable like the atoms of the atmosphere,) are but about one-fourth of the original number; whereby each ball of this smaller number will have to sustain nearly four times the *proportional* inductive influence before sustained by each ball of the larger number.

(62.) By the same change, the number of balls in the first stratum round the hemispherical part of the cylinder, will be reduced to one-sixteenth of the original number; whereby each ball of this smaller number, will have to sustain a far greater *proportional* inductive force or influence than that before sustained by each ball, of such original or larger number; and also a far greater *proportional increase* of induction than that sustained by the reduced cylindrical portion of the cylinder.

(63.) A great increase of tendency will therefore be acquired, by the reduced hemispherical end of the cylinder (if electrified positively), to part with its electricity to the stratum of balls next adjacent to it (prop. 15, pp. 10, 11, 12), particularly those about h , where the positive state of the cylinder will be at the highest; and where also the nearest balls will be in the most negative state on the side nearest the cylinder, and the most positive state on the remoter side. When this positive state of the cylinder, and negative state of the nearest half of the balls, (or, which I suppose the same thing, (34,) the atoms of a surrounding atmosphere,) become so great, that the attraction of the balls or atoms for the electricity of the cylinder overcome that of the cylinder itself, a portion of electricity will pass as a spark from the cylinder, to a ball or balls (or atoms) about h (11, 12).

(64.) These electrified balls (or atoms) will then, for an instant, be in a similar situation, with respect to the electrified cylinder, as each of the electrified balls in figs. 5 and 6, is to the other; and will, for the reasons there explained, recede from the cylinder. Other next adjacent balls or atoms will supply their places; become similarly electrified, in the same manner, recede to make room for others, and so on as long as the electricity is sufficiently intense, to pass in a spark to the balls or atoms about h .

(65.) The converse, so far, applies pretty accurately to the negative state of electricity.

(66.) There is, however, a peculiarity in the positive discharge, from a point into the atmosphere not applying to the negative; the former giving a brush-like mass of light, the latter a light generally compared to a star; the explanation of which appears to me to be indirectly connected with the doctrines of the recession.

(67.) The chief cause of the difference I apprehend to be, that in the former case, a large reservoir of electricity, containing more than its natural share, is pouring itself upon a number of smaller ones, containing already their natural share; while in the case of negative electricity, several small reservoirs, containing their natural share of electricity only, are pouring that small quantity into a large reservoir deprived of a portion of its natural share. In the

former case, the absolute quantity of electricity in motion, from any one atom to any other of the air, is far greater than in the latter. In the latter case, the natural quantity, at most, is in motion ; in the former, the natural quantity, with a considerable addition.

(68.) When the first atmospheric atom at h , on the line $h o$ (fig. 4), reduced as before mentioned, and representing the end of a long blunt pointed body, has become *positively* electrified, it acts for an instant like a most attenuated point to the electrified body ; its mobility being far less than that of electricity, (and electricity, I suppose, possessing homogeneous attraction and momentum), more of the electricity is enabled to follow the first portion, and pass on over such first atmospheric atom before it has had time to recede, using it as a stepping-stone to the second ; that to the third, and so on, as to other more distant atmospheric atoms, about the line $h o$, according to the intensity of the electrification, the size of the point, &c.

(69.) These electrified atoms, having receded the breadth of one atom, a new one has also been simultaneously drawn into the place of the first ; serves in like manner, as a stepping-stone to the stream of electricity ; in like manner recedes, and so of others.

(70.) At different distances, (increasing with the increase of the rounded end of the body), this principal spark branches out into several ramifications, to which the above observations, also partly apply.

(71.) When the same body (fig. 4), reduced as before mentioned (60,) is electrified negatively, the quantity of electricity passing at once from the atmospheric atoms into the negatively electrified body, is limited to the small *natural* quantity in the first few strata of atoms ; for, though the first stratum may act sufficiently, by induction, to draw sparks from the second stratum, the second from the third, &c., yet these effects must be less than in the case of positive electricity, and be rapidly diminished by distance and the consequent increase in the number of atoms, in the remoter strata, to be acted upon, by induction, by the smaller number in the nearer strata. The line of sparks, or stream of electricity, must be limited to the natural quantity, at the most, belonging to the atmospheric atoms through which it passes, while the stream from a positive point, to the line of atmospheric atoms, through which it passes, (and even its ramifications,) might well be conceived to be many times larger, and consequently able to act inductively upon, and pass in a continuous spark to, a much more distant stratum of atmospheric atoms, especially when aided by the concentrating power of its own homogeneous attractions.

(72.) The negatively electrified body, appearing from the deficiency in the quantity of electric fluid to be incapable of thus transmitting its negative state to, or rather of drawing the electric fluid from, any great distance, even by using the atmospheric atoms as stepping-stones, it is necessary that every atom, in order to be-

come electrified, should approach near to the point, and transfer its small contribution, either immediately to the point or to some atom very near it.

(73.) The atoms near to, and immediately in front of, the point, having become negative in a degree, according to their distance, and incapable of further transmission of electricity by spark; and being more immobile than the electricity, the atoms around the point, and around those atoms nearest to it, and already electrified, pour their electricity into the point (and perhaps into such nearest atoms in front of it), at all or nearly all angles, with the axis of the long conducting body, and then recede, and pass off in the stream of electrified particles, whose general direction is, in the axis of the body, prolonged.

(74.) The view I have taken, that the lesser depth of film is the cause of the less free and distant discharge of negative electricity, is favoured by some variations I tried on the common experiment of piercing a card with the positive and negative points of the discharger, placed at a distance from each other, in contact with the opposite surfaces of the card.

I have for some years supposed that the current of a battery, in all cases, begins both from the positive and negative surfaces simultaneously; so that one end of the connecting conductor becomes negative by deduction from its natural portion of electricity by the negative surface of the battery with which it is in contact, and the other end positive, by an additional portion of electricity, received by it from the positive surface with which that is in contact, and that the conductor would offer a less resistance to the deeper or positive current, so that the meeting of the two states in the conductor would be nearer to the negative than the positive surface of the battery. I also supposed that the same causes, operating more decidedly to the disadvantage of the negative state of electricity on an imperfect conductor, like a card, might be the cause why the perforation was always at the negative point. With these views, and in order to give the negative conductor time to abstract a portion of the shallow natural film of electricity from the negative surface of the card, before the positive surface should be affected by additional electricity from the positive point, I made the connection between the negative point and the battery, a *few inches only* in length, and also insulated, while that between the positive point and the battery was *many feet* long, and connected with a fender, the floor, &c. &c. I also made the card damp, to facilitate the negative electrification of the surface. On making the discharges, I obtained perforations of the card at above an inch from the negative point, when the points were $2\frac{3}{4}$ inches apart. Height of intensity, as might be expected, also helps this removal of the perforation from the negative point.

(75.) The electrical fly is but the converse example of the recession of electrified atmospheric atoms from a point. The points

of the fly continually electrifying a number of atoms as above-mentioned, just in front of them, and yet continuing replenished with electricity, each point answers to one of the balls in figs. 5 and 6, and the electrified atoms in front of it, to the other ball in the same fig. While the unelectrified atoms, at a distance in front of the point, attract the electrified atoms between them and the point, tending to draw such electrified atoms in that direction, the unelectrified atoms behind the point attract its electricity, and consequently itself, in the opposite direction.

(76.) The mass of electrified atoms from which each point recedes, being continually replaced by others equally electrified, an acceleration of the receding motion takes place, until the motive force is equalled and balanced by the friction of the air and the pivot of the fly.

(77.) I may be allowed to conclude with a hope that my labours may result in sufficient utility to form an excuse, at least, for the imperfections I feel sensible will be found in their execution.

CHARLES GRIFFIN.

LEAMINGTON, 29TH JUNE, 1839.

NOTE TO PROP. 58.—*Coulomb's Law*, though it may express the attractions between two balls, must not be depended on as a true expression of the forces exerted by electricity on other matter (inductively or otherwise) at a distance, as his balls allowed of a transference of the fluid from one part of the surface to another, thereby changing the position of the centres of attraction. I had begun to prepare for some experiments to ascertain the true forces of attraction, when I was interrupted by ill health, which has added to my natural inaptitude for delicate manipulations; but should be glad to see the matter taken up by some such ingenious and dexterous manipulator as Mr. Snow Harris. It may not be amiss to state here my intended apparatus, &c., as a suggestion to others, viz. :—

First, A perfectly spherical ball, with metallic surface, and insulated by a glass leg, coated with melted wax.

Secondly, A hollow sphere, of rather larger diameter, to inclose the other sphere or ball, their centres coinciding, and consequently their opposed surfaces parallel: this sphere, however, divided into hemispheres for the convenience of removal. A small well rounded hole in this hollow sphere, rather larger than the glass leg of the inclosed ball, for the admission of that leg without contact. A portion of the hollow sphere to be also so cut out as to be capable of admission, as accurately as possible, without actual contact. The piece so cut out to be suspended from the arm of

Thirdly, A delicate balance. A scale at the opposite end of the balance beam, and the whole exactly poised.

B

Fourthly, Small weights for this scale (*query*), cut from fine wire in multiple lengths.

It is evident that electricity, on the smaller sphere or ball, would exert equal forces on equal spaces of the larger or hollow sphere, including that part of it cut out and suspended from the arm of the balance.

Let the hollow sphere be, say, half an inch from the electrified one; and the attraction being ascertained for the piece affixed to the balance, let the electrified ball be inclosed in another hollow sphere, which shall be, say, an inch from the electrified one, and have a piece cut out and affixed to the balance, bearing the same proportion to the whole as did the piece cut out of the first hollow sphere. The attraction of the *same* quantity of electricity as was used for the smaller hollow sphere, having been ascertained for the piece cut out of the larger, the proportion they bear to each other will indicate the relative attractions of the *same portion* of electricity at *different distances*. Should the attraction be the same for each piece cut out, then a similar attraction will be equal at all distances, and Coulomb's law will apply to this case. I think a whole hemisphere might be conveniently affixed to the balance, instead of cutting out a smaller piece for that purpose, unless (which I think would be best, and perhaps almost necessary,) the electrified sphere should be at least a foot in diameter, and the two hollow spheres of very different sizes.

In the experiments of Coulomb, and those now suggested also, the attractive influence is exercised by induction, or through the medium of other bodies. What it may be between two bodies without any intermedium (as two atmospheric atoms are by others supposed to be), is another question.

By the arrangements above suggested, might also the inductive attraction of *different* multiple and other *portions* of electricity, for solid matter at the *same distance* be ascertained, and perhaps also the forces called repulsive.

Note on the relative Measures of the powers of a Copper-zinc and a Platinum-zinc Voltaic Series. By Dr. M. H. JACOBI, Mem. Imp. Acad. St. Petersburg.†

I have the honour of communicating to the Imperial Academy of Science, the result of my experiments on the relative powers of two different kinds of voltaic cellular series; the one composed of copper and zinc, charged with sulphate of copper and sulphuric acid, diluted with six times its volume of water; the other, platinum and zinc, charged (after Grove's method) with concentrated nitric acid and sulphuric acid, similarly diluted to that in the former series. The copper-zinc series had 38, and the platinum-zinc series only 2.5 square inches of surface. For the measure of the power, I availed myself of the Balance of Becquerel.* This instrument is

* M. Becquerel's Balance is fully described in the "Annals of Electricity," &c., vol. 1. page 398.—EDIT.

† Poggendorff's Annalen der Physik und Chemie.

valuable for accurate measurements, when the wire cylinders are so contrived that they for a counterpoise, are enabled to make the necessary conditions of equilibrium, which is attained when the wire cylinder can so act as to repel the magnetic bar. For this purpose, one of the cylinders must be fastened below that bar, and the other over it. Through the latter hollow cylinder passes a pin or wire, by means of which the magnetic bar is suspended from the arm of the balance. It is necessary, however, to apply a *correction* to the indications afforded by this instrumental measurement of the powers of the currents. This correction, the necessity of which has been shewn by other contemporaneous labours, is in the proportion of the squares of the powers of the currents. If, for instance, the *absolute* power of the current be represented by k' , and the power indicated by the measuring instrument by k , we shall have this equation:

$$k = k' - y k'^2, \text{ from which we obtain } k' = \frac{1}{2y} (1 - \sqrt{1 - 4ky})$$

From a series of observations, I found for my equipoise—

$$y = 0.00004228.*$$

The following table contains the experiments made with the before-mentioned voltaic series. The first column shews the resistance L , offered by the connecting wire which served as the spiral, a resistance which was ascertained by other experiments.

The other two columns contain the true powers of the currents; that is, the measured powers, corrected by the above formula:—

L.	POWERS OF THE CURRENTS.	
	Copper-zinc.	Platinum-zinc.
23.1	0.380	0.395
135.3	0.097	0.135

If A and A' represent the electromotive powers, and λ and λ' the resistances presented by the series themselves, we shall have, according to the formula of Ohm,† these four equations—

$$\frac{A}{\lambda + 23.1} = 380$$

$$\frac{A}{\lambda + 135.3} = 97$$

$$\frac{A'}{\lambda' + 23.1} = 395$$

$$\frac{A'}{\lambda' + 135.3} = 135$$

$$\text{Therefore } \begin{cases} A = 14610 \\ \quad = 15.35 \end{cases} \quad \begin{cases} A' = 23,000 \\ \lambda' = 35 \end{cases}$$

* Poggendorff's Annalen, Bd. xlvii., s. 225.

† We propose to give Professor Ohm's Theory of Electric Currents in an early number.—EDIT.

Or, if we take λ , which, in this case, is 36 square inches, for the unit of surface,

$$\lambda' = \frac{35 \times 2.5}{36} = 2.4$$

If s represent the whole surface of the pile, z the number of pairs, c the strength of the current, L any resistance,

$$\text{Then, } C = \frac{z A s}{z^2 \lambda + L s}$$

From this equation, we learn that the maximum of power of the current is obtained, when the pile is so managed, that $\frac{z^2 \lambda}{s} = L$; that is, when the whole resistance of the pile is equal to that introduced into the pile itself, in addition to the foreign resistance of the conductors. Hence, for any other arrangement than that which answers to a maximum of action, no constant relation between the different voltaic combinations are extant; nor can we, under any other circumstances, form a correct judgment of their relative superiority. By the elimination of z we have—

$$C (\text{max.}) = \frac{A \sqrt{s}}{2 \sqrt{\lambda L}} = \frac{A' \sqrt{s'}}{2 \sqrt{\lambda' L'}} :$$

from which, after substitution of the former for A, A', λ, λ' , their values are found to be—

For the whole surface $s' = s \cdot 0.06$,

And in reference to the number of pairs $z' = z \cdot 0.6$;

that is, *we need only a pile of six square feet of platinum surface, in order to supply the place of a pile of a hundred square feet of copper surface*; or, in reference to the number of pairs:—*Six pairs of platinum, of a square foot of surface each, give the same action as ten pairs of copper of ten square feet of surface each.* This extraordinary superiority of platinum has been well confirmed by other experiments.*

* At the time I received the above note, its author sent me a leaf of the *Journal de St. Petersburg*, in which he states that the electro-magnetic powers of a Grove's battery would become applicable to navigation. The battery of which he speaks, consists of 64 pairs of platinum plates, each plate 36 square inches of surface; so that the whole platinum surface is 16 square feet. This is probably the mightiest battery that has hitherto been constructed, the great battery of the London Institution, of 2000 pairs of plates, and 1000 square feet of surface, not excepted. Notwithstanding the extraordinary powers of its thermo-chemical and dynamic action, the physiological effects of this great battery, as shown by Schönbein's small one of six pairs of plates, are only feeble. The action on the human body is discernible, but is easy to bear. We must remark, in this place, that the author does not look upon this phenomenon as a specific quality of the platinum battery, nor as an anomaly; since it finds an explanation in Prof. Ohm's mathematical laws of the galvanic battery.—*Poggendorff*.

On the Observations of M. BECQUEREL, relative to the Comparative Measures of Action of two Voltaic Series; the one Copper-zinc, the other Platinum-zinc. By Dr. M. H. JACOBI, Mem. Imp. Academy St. Petersburg, &c. &c.

At the sitting of January 13th, of last year, I had the honour of presenting a note to the Academy, on the measurement of the comparative action of two distinct voltaic series, the copper-zinc, and the platinum-zinc. This note has given rise to some observations by M. Becquerel, at the Academy of Sciences, Paris, on the 4th January last, in which he thus expresses himself:—

“It seems from this conclusion, that in piles in general, platinum is far superior to copper for transmitting large quantities of electricity. But this superiority, in the present case, is due to a condition which M. Jacobi has not noticed, although it exercises a considerable influence on the action of the pile, which is sometimes neglected by experimentalists.”

M. Becquerel attributes the superiority of the platinum series to the reaction of the concentrated nitric acid upon the acidulated water, which gives rise to an electric current, of much more considerable energy than that resulting from the solution of sulphate of copper on the similarly acidulated water in the copper series.

M. Becquerel then adds, “I thought it my duty to make these observations, which in other respects are not new, in order that experimenters may not be led into error respecting the causes of the effects obtained by M. Jacobi.”

From this observation, one might suppose that, in my note, there was a very serious omission, since M. Becquerel thought it his *duty* to fill it up. The calculations from my experimental data were made accordingly with the formula of Ohm; and the result shows, clearly enough, the cause of the effects: but to this formula I have thought proper to introduce the expression of the maximum of effect, because it is this expression (till then not noticed) that places us in circumstances which enable us to make accurate comparisons on the action of different voltaic combinations. It is with pleasure that I embrace this opportunity of declaring myself in favour of the theory of M. Ohm, which, in my opinion, is one of the most extraordinary advances in this department of natural philosophy. It collects into one view an immense assemblage of facts; and explains distinctly the whole of the phenomena which relate to the force of voltaic currents. Indeed, this theory has been confirmed and extended by some of the most distinguished philosophers. The law of Ohm, known in Germany for more than thirteen years, is now beginning to spread itself over England, and will give, I hope, a new direction to the zealous experimentalists of that country. In France, the same law, ten years after its publication, was announced as a new discovery, and has been received as such.

Respecting the point at issue, scarcely knowing in what manner to submit to calculation the chemical reaction of which M. Becquerel speaks, I have contented myself, according to observations, to account for the superiority of Mr. Grove's battery.

1. Because, in the same transverse section, the resistance in this pile is to that of the copper-zinc as $2\cdot4$ $15\cdot35$; and

2. Because, under the conditions in question, the electro-motive force of platinum-zinc is to that of copper-zinc as 23000 to 14610.

These numbers, which result from the relation of resistances, do not present any thing remarkable, as concentrated nitric acid has long been known as one of the best conductors, and the resistance to transmission is lessened when the metals are immersed in strong acids.

As to the electromotive force, excited principally by the contact of two heterogeneous metals, there enters, without doubt, in part, the force resulting from the contact of the two heterogeneous liquids, or, if you please, from their mutual chemical reactions. The existence of such an action has, for some time, been questioned, and it is some recent experiments only that has placed it beyond doubt. But this is not what M. Becquerel seems to allude to, for it may be shewn that this latter effect is so inconsiderable, that the superiority of the platinum-zinc might rather be attributed to any other cause. The very opposite opinion to that entertained by M. Becquerel, appears to be favoured by the pile that he himself has discovered, and which now bears his name. This pile is well known to be composed of two platinum plates, one immersed in concentrated nitric acid, and the other in a solution of caustic potash.* Here are two metals, in *appearance* the same, and the circumstances are the most favourable for chemical reaction between the nitre acid and the alkali, which, without doubt, is far more powerful than that between nitric acid and diluted sulphuric acid. But M. Fechner has proved, by the most careful experiments, instituted without prejudice, and with the skill and accuracy which characterise all his operations, that if we represent the total force of a pile of this construction, by the number $8\cdot644$, the share of this force, due to reaction between the acid and alkali, will only be $0\cdot140$, or about a sixtieth part of the whole. This numerical result appears rather unfavourable to M. Becquerel's explanation, and will be still more so if we compare the action where the platinum is placed in the alkaline solution, with that which is brought into play where the zinc is placed in the acidulated water. It is to be hoped that M. Fechner, who is dexterous in the manipulation of these experiments, and who is possessed of the necessary apparatus, will examine this business still further.

It appears to me, from the present state of our knowledge, that

* We believe that Sir H. Davy was the first philosopher who employed piles or batteries of this kind.—EDIT.

chemical action can scarcely be admitted as the only source of voltaic phenomena; nevertheless, it is one of the chief agents,* and I am not afraid to state, briefly, some well established facts, which, to me appear to explain in what that agency consists:

1. A voltaic current can only exist by the contact of heterogeneous metals, or, in general, by the contact of dissimilar bodies.

2. Limiting our reasoning to metals—a class of bodies from which we derive the most unequivocal results—there can be no pair of them, unless perfectly homogeneous, that will not produce an electric current when immersed in one and the same liquid. Hence, whenever a current is produced, indicated even by the most delicate galvanometer, the metals employed are not perfectly homogeneous.

3. Metals become heterogeneous by the slightest alteration on their surfaces, either by mechanical or chemical agency; and the latter may be so slight as not to be ascertainable by any chemical test whatever. Even the catalytique forces of Berzelius may be sufficient to produce heterogeneity, indicatable by a galvanometer, on the surface of metals otherwise perfectly homogeneous.

Keeping in view these facts, it will be easily understood that two pieces of metal of the same kind, although perfectly homogeneous, can no longer be so after immersion in two different kinds of liquid. Hence it is, that the platinum which is placed in the nitric acid, when voltaically considered, is a different metal to the platinum placed in the alkaline solution. This superficial change, which is often accomplished in an infinitely short period of time, sometimes augments or raises the natural electromotive relation of metals, and sometimes diminishes, or even completely destroys it. The current produced by contact is generally, perhaps *necessarily*, accompanied by a chemical decomposition,† the results of which exercise a certain influence on the surfaces of the metals. This influence, I say, has always a tendency to neutralise, by compensation, the electromotive difference of the metals, to enfeeble the current, and, in time, to completely annihilate the action of the pile. By the batteries with separate compartments for each metal, a most excellent discovery of modern date, we can sustain or prolong the current at a constant rate of force, by preventing the liberation of the noxious bodies; an effect which cannot be obtained by the ordinary piles, in which one liquid only is employed.

4. A certain relation between the conductivity and the chemical condition of liquids is generally observed, although no decisive opinion on this point can as yet be formed.

This acknowledgment is sadly at variance with the previous conclusions of Jacobi, drawn from the experiments of Fechner!!!—EDIT.

† It is rather singular that our author, who we should suppose to be well acquainted with the simple plates of Volta, could arrive at such a conclusion. A moment's contact produces a momentary electric current. And by the dry electric piles, currents of twenty years, nay, of hundreds of years duration, may be produced, and still no chemical action, *within the pile*, taking place.—EDIT.

5. It appears, from various facts, that chemical action diminishes the resistance to transmission.

From this interpretation, we easily perceive the importance of the influence which chemical action exercises in the production of voltaic phenomena. Nevertheless, this influence is not primary, but secondary only; and, in the greater number of instances, the facts lead to the conclusion that voltaic currents are producible, not *because*, but *although*, chemical action is present.

In the closed circuit, the electro-chemical, the electro-thermo-metric, and the electro-magnetic effects, exist simultaneously, by the same law, and in the same proportion.* If at some future period it should be found possible to express, by similar units, these effects, so dissimilar, the force generated and sustained by contact may possibly be found to be a quantity as constant as is the active force of a system of real points in a state of motion. We should, then, accordingly with the different manifestations of the current, have to turn our attention to the production of useful results only, in the same manner as in the mechanical arts, where the greatest pains are taken to render the active force as available as possible, so that no part of it may be unnecessarily wasted. I take the further liberty of presenting to the Academy the following experiments, which I have found in my journal, and which form the sequel to those described in the note in question.

After I had completed those experiments, the piles that had been employed in them remained charged, but out of action for fourteen hours. I afterwards repeated the measurements, and the following numbers were derived from them:—

Resistance of helix.	Force of Copper-zinc plates.	Force of Platinum-zinc plates.
23·1	385	595
135·3	92	133

I must remark, that the fluctuations of the force, which took place, prevented me from taking the measurements with much precision. The calculations, according to the formula, give—

	Copper-zinc.	Platinum-zinc.
Electromotive force	13552	22515
Resistance	12·1	33·9

The pair of copper and zinc having 36, and that of platinum and zinc only 2·5, square inches of surface, it follows, since the resistance is inversely as the transverse section, that the resistance for a platinum plate of 36 square inches surface will be only

$$\frac{33\cdot9 \times 2\cdot5}{36} = 2\cdot3$$

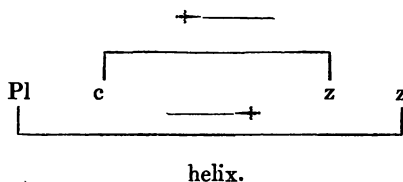
* It is impossible to imagine how a philosopher, of Jacobi's experience, should have fallen into so obvious an error. EDIT.

If s express the total copper surface of a pile, and s' the total platinum surface of another pile; and z and z' represent the number of pairs which give maximum effects in each pile respectively, we have

$$\begin{aligned} s' &= s \dots\dots 0\cdot07 \\ z' &= z \dots\dots 0\cdot6 \end{aligned}$$

which differs but very little from the numbers first obtained.

To confirm, in some measure, these results, I formed a circuit of these two dissimilar pairs, according to the following arrangement, so that the two currents traversed the helix, whose resistance was 23·1, in opposite directions:—

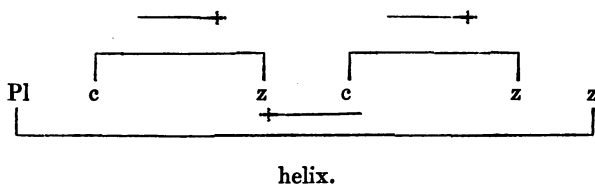


The force of this combination was in favour of the platinum 1335 gr. Making the calculation, we obtain

$$\frac{22515 - 13552}{23\cdot1 + 12\cdot1 + 33\cdot9} = .129\ 7\ \text{gr.}$$

which differs from the observation by ·0048 gr.

I afterwards formed the circuit in the following manner:—



In which two pairs of copper-zinc, in series, were opposed to one pair of platinum-zinc. The current was opposite to that in the former experiment, being in favour of the copper-zincs. The electromotive force of the additional pair was not previously ascertained, nor was it possible to obtain exact measurements, because of the continual oscillations of the balance. I availed myself of a moment's steadiness, however, which showed ·043 gr. for the force

of the current. Supposing the two pairs of copper-zinc to be equal, we should obtain, by calculation—

$$\frac{2 \times 13552 - 22515}{23.1 + 2(12.1) + 33.9} = .056 \text{ gr.}$$

Which differs somewhat from the observation. Nevertheless, this experiment proves that, from the numbers obtained, the superiority of a platinum-zinc pair is not sufficiently great to balance a current excited by two copper-zinc pairs in series. No advantage would be obtained by increasing the surface of the platinum, because that would merely alter the value of the denominator. The sign of the numerator would not be affected.*

* It appears to us that some errors have crept into this paper.—EDIT.

A Thermo-Electric Observation; communicated in a Letter from H. Councillor MUNCKE, to Prof. Poggendorff.*

“Recently, a stranger shewed to me the following very odd experiment:—Bring one end of the wire of a thermo-mulhplier (galvanometer) into connection with one end of a bar of bismuth, and the other end of the wire into connection with a bar of antimony. If, now, we bring the other ends of the two bars either into simple contact, or push them forcibly together, the needle moves towards the *east*; but if we rub the ends of the bars against each other, the needle moves towards the west. The thing seems strange, and deserves consideration, but I have not succeeded to make out the explanation from these crude experiments.”

The same experiment was also shewn to me by M. Wartmann, of Lausanne, during his late visit to Berlin, and I have since that time, on several occasions, convinced myself of its accuracy. The explanation of the phenomena appears to be pretty simple. When, with the fingers, we squeeze the antimony and bismuth towards the ends next to the galvanometer, those places where the fingers rest become warmer than the ends of the bars which are simply brought into contact with each other. But, on the contrary, when we *rub* the same ends of the antimony and bismuth against each other, the heat becomes greater there than at those places held by the fingers. As the difference of temperature changes, the direction of the current necessarily changes also.—POGGENDORFF.

* From Poggendorff's *Annalen der Physik und Chemie*, No. 2, 1841.

An Account of the Magnetic Observations made at the Observatory of Harvard University, Cambridge, by JOSEPH LOVERING, Hollis Professor of Mathematics and Natural Philosophy, and W. CRANCH BOND, Astronomical Observer to the College.*

(*Memoirs of the American Academy.*)

The object of this communication is to give some information in regard to the nature and progress of the series of magnetic observations which has been instituted at the Observatory of Harvard University, in co-operation with the plan of the Royal Society, as detailed in their reports.† Regular magnetic observations were undertaken at Cambridge, in March, 1840, being confined at first to the monthly term-days of the English scheme. The meteorological observations, recommended in the same reports, were also begun in a modified form, adapted to the resources of the Observatory at that time; but it is not proposed to speak of them, except so far as they may be connected with the remarks on terrestrial magnetism. The necessity of conforming, as near as possible, to the general plan, and acting in concert is apparent, as the observations are intended rather for comparison than for independent use. But in the execution of this charge, as far as it has yet progressed, questions of curiosity or of higher interest have been constantly occurring, which suggested the expediency of observations additional to, but not interfering with, the chief work. In some cases, these inquiries could be answered, or the approximate data for a solution be derived, from the means at our command; but often they depended upon comparisons between elements simultaneously observed at different places. In the latter instances we have had valuable assistance in the prompt co-operation of Lieutenant C. J. B. Riddell, at the Magnetic Observatory in Toronto, U. C. The results of these extraordinary observations will be exhibited in their proper place. That an opportunity may be afforded of judging of the degree of accuracy of the observations, we shall first give a description of the plan of the Observatory, the nature, position, and adjustment of the instruments, and the method of making the observations.

Plate I. is a ground plan of the University Observatory. The projections are given of only those parts of the building which are devoted to the purposes of an observatory; they were built expressly for this object, and are attached to the north-west and south-west

* The authors of this paper desire it to be understood, that Professor Benjamin Peirce has rendered great assistance in conducting the observations on term, and other days, and in devising simple methods of reduction. The empirical curves which are mentioned in this paper, and which were calculated at a great cost of time and labour, are the voluntary contribution of this gentleman to the objects of the Observatory.

† Royal Society. Report of the Committee of Physics, including Meteorology, on the Objects of Scientific Inquiry in those Sciences. London, 1840. — Report upon a letter addressed by M. Le Baron de Humboldt to his Royal Highness the President of the Royal Society, and communicated by his Royal Highness to the Council 9th June, 1836.

corners of the house in which the observers reside : one room of his house, which is used for an observer's room, is represented in the plate by *K*. *A* is the projection of the dome erected on the top of the house, and the rectangle is the opening through the floor by which it is entered. This dome has a revolving roof of panel work and plate glass, with a vertical section which can be brought into any azimuth required by the observation. *E* is the place of the astronomical clock.* It has been thought inexpedient to carry the clock into the transit room, where it would be exposed to great changes of temperature. For this and other reasons it is kept in a fixed position, in a room where the temperature is nearly uniform throughout the day and night, and at all seasons. The observations are made directly with the chronometer, which is compared with the clock as often as is necessary. This practice has recommended itself, from long trial, as convenient and safe. *G* represents the place of the barometers. The observatory is liberally furnished in this respect, having one standard barometer by Cary, and two barometers by W. and S. Jones. Besides these, a beautiful standard barometer, made by I. Newman, has just been received, and has been placed between the windows, on the south side of the same room. As this barometer is after the model of those used at the other magnetic stations, and has been compared directly with the standard barometer of the Royal Society, which was made by the same artist on the same principle, it may be regarded as the best standard in this country. The following are the results of the comparison :—

Royal Soc. Standard, 29.506 in.	Royal Soc. Standard, 28.604 in.
Cambridge Standard, 29.504	Cambridge Standard, 28.600

Newman's barometers are superior in several respects to all others. The graduated scale which measures the height of the mercury is made of brass, and slides by means of a tangent screw, so that its end, which is of ivory, can be made to touch the upper surface of the mercury in the cistern. This is known to be the case when the reflected and actual point are in contact. The tube which holds the mercury is 0.512 of an inch in diameter; therefore the effect of capillary attraction is inappreciable.† The Royal Society have published, in their Report, a table of corrections for temperature, calculated by Professor Schumacher, and applicable to this kind of barometers, by which an allowance is made for the unequal expansion of the scale, and the mercury corresponding to every degree from 32° of Fahrenheit and for every half inch of the barometer. The vernier of Newman's standard barometer reads off to $\frac{1}{500}$ of an inch.

The piers which support the transit instrument are situated at *B*, and may be better understood from *m n o* in another part of the plate. The piers are $9\frac{1}{2}$ feet long, with a tripod base; *m* is the east

* This clock was made by Parkinson and Frodsham.

† This correction for a tube whose diameter is 0.5 inch is only 0.003 inch.

and west view, and *n* the north and south view; *o* is a granite pedestal, 6 feet long, $5\frac{1}{2}$ feet wide, 2 feet 10 inches deep, firmly resting upon a gravel foundation. The lower side of this block of granite is $7\frac{1}{2}$ feet below the floor. The sides of the transit room are supported on independent walls, separated from the base of the transit by a trench 3 feet wide, which has been filled up with tan to preserve the stones from being deranged by frost. Care has been taken not to allow the floor to press upon the piers where they enter it. By these precautions, the instrument is protected from the jar of footsteps in the observatory, and the disturbances of carriages. An excellent Transit-instrument, made by Troughton and Simms, 4 feet long, was placed in adjustment in January, 1840, and a series of meridian observations, including moon-culminating stars, has been continued from that time, which, by their comparison, afford satisfactory evidence of the stability of the pillars on which the instrument rests. *FF* is the direction of the astronomical meridian, and intersects Bluehill, in Milton, west of its summit. A firm and substantial meridian mark has been erected on that hill, consisting of a tower of round and substantial masonry, thirteen feet in diameter at the base, seventeen feet high above the ground, and nine feet in diameter at the top. On this is placed a mark seven feet high, of the shape of the rhomb, with its larger axis perpendicular to the horizon. By this means the central vertical wire of the transit instrument is put in the meridian. It appears from Mr. Borden's State Survey, that the distance between the mark and the instrument is 58,520.5 feet. A brick house on the meridian line, about a mile from the transit instrument, affords a convenient, though less accurate reference, when the state of the atmosphere does not allow the distant mark to be distinctly seen.

A short abstract from the astronomical records of the Observatory is annexed, to show the state of the transit instrument, and the confidence that may be reposed in the accuracy of the time used in the magnetic observations. The equatorial intervals of the wires of the transit instrument, as deduced from a large number of observations, are thus:—

	s.
From 1st wire to the mid-wire . . .	33.96
From mid-wire to the 5th . . .	33.94
From 2nd to mid-wire	16.88
From mid-wire to 4th	16.86

The following extract from the transit book includes all the standard stars, whose transits were observed from July 10th to July 25th, 1840. In a few cases, an observation has been rejected because one of the five wires was accidentally missed. The introduction of such stars would vitiate the general comparison.

No.	Date. 1840.	NAME OF STAR.	Declination of Star.	Obs'd time of transit by the sidereal clock.			Right Ascension of Star.	Clock slow of sidereal time.		Difference of obs'd times— Diff. of A.R.*
	July.		° ' "	h.	m.	s.	m.	s.		s.
1	10	Polaris S.P.	+88 27	13	00	57.3				
2	"	α Virginis	—10 20	13	15	38.84	48.56	— 1	9.72	
3	"	δ Bootis	+27 45	14	36	53.14	2.64	— 1	9.50	+ 0.29
4	"	α^2 Libræ	—15 23	14	40	55.50	5.10	— 1	9.60	— 0.10
5	"	β Libræ	— 8 48	15	7	17.60	27.19	— 1	9.59	+ 0.03
6	"	α Cor. Bor.	+27 15	15	26	48.02	57.68	— 1	9.66	— 0.05
7	"	α Serpentis	+ 6 56	15	35	16.84	26.37	— 1	9.53	+ 0.14
8	"	β Scorpii	—19 22	15	55	02.48	11.90	— 1	9.42	+ 0.13
9	"	δ Ophiuchi	— 3 17	16	3	51.52	61.19	— 1	9.67	— 0.25
10	"	Antares	—26 4	16	18	30.42	40.10	— 1	9.68	— 0.00
11	"	γ Scorpii	—27 52	16	24	49.92	59.72	— 1	9.80	— 0.11
12	"	α Herculis	+14 35	17	6	14.86	24.47	— 1	9.61	+ 0.22
13	"	γ Draconis	+51 31	17	51	46.94	56.63	— 1	0.69	— 0.05
14	"	Polaris	+88 27	1	0	59.80				
15	11	α^2 Libræ	—15 23	14	40	53.87	5.09	— 1	11.22	
16	"	β Ursæ Minoris	+74 48	14	50	06.59	18.27	— 1	11.68	— 0.46
17	"	β Libræ	— 8 48	15	7	16.05	27.18	— 1	11.13	+ 0.56
18	"	α Serpentis	+ 6 56	15	35	15.03	26.36	— 1	11.33	— 0.18
19	"	β Scorpii	—19 22	15	55	0.59	11.90	— 1	11.31	+ 0.03
20	"	δ Ophiuchi	— 3 17	16	4	49.95	61.18	— 1	11.23	+ 0.08
21	"	Antares	—26 4	16	18	28.67	40.10	— 1	11.43	— 0.19
22	"	γ Scorpii	+14 35	16	24	48.41	59.72	— 1	11.31	+ 0.12
23	"	α Herculis	+12 41	17	7	13.09	24.46	— 1	11.37	— 0.03
24	"	α Ophiuchi	—27 52	17	25	22.45	33.80	— 1	11.35	+ 0.03
25	"	γ Draconis	+51 31	17	51	45.11	56.62	— 1	11.51	— 0.14
26	12	β Libræ	— 8 48	15	7	14.90	27.18	— 1	12.28	
27	"	α Serpentis	+ 6 56	15	35	14.06	26.36	— 1	12.30	— 0.00
28	"	β Scorpii	—19 22	15	54	59.52	11.89	— 1	12.37	— 0.06
29	"	δ Ophiuchi	— 3 17	16	4	48.98	61.18	— 1	12.20	+ 0.18
30	"	Antares	—26 4	16	18	27.88	40.09	— 1	12.21	— 0.00
31	"	α Hercules	+14 35	17	6	12.02	24.46	— 1	12.44	— 0.20
32	"	Sagittarii	—30 14	17	47	40.84	53.07	— 1	12.23	+ 0.24
33	"	Vega	+38 38	18	30	22.04	34.37	— 1	12.23	— 0.06
34	"	ϕ Sagittarii	—27 09	18	34	31.52	43.76	— 1	12.24	+ 0.09
35	14	α Herculis	+14 35	17	6	10.16	24.45	— 1	14.29	
36	"	α Ophiuchi	+12 41	17	26	19.76	33.79	— 1	14.03	+ 0.27
37	"	μ Sagittarii	—21 6	18	3	01.46	15.67	— 1	14.21	— 0.16
38	"	α Lyræ	+38 38	18	30	20.12	34.37	— 1	14.25	— 0.02
39	"	β Lyræ	+33 11	18	43	59.16	13.55	— 1	14.39	— 0.13
40	"	ζ Aquilæ	+13 38	18	56	52.48	6.72	— 1	14.24	+ 0.16
41	"	δ Aquilæ	+ 2 48	19	16	14.08	29.30	— 1	14.22	+ 0.04
42	"	α Aquilæ	+ 8 27	19	41	47.82	61.98	— 1	14.16	+ 0.07

* This difference has been corrected for the error of the clock. This error is found from the 7th column of the table, and may be seen on page 32.

No	Date 1840.	NAME OF STAR.	Declination of Star.	Obs'd time of transit by the sidereal clock.			Right Ascension of Star.	Clock slow of sidereal time.		Difference of obs'd times— Diff. of A.R.
	July.		°	h.	m.	s.	m.	s.		s.
43	14	β Aquilæ	+ 6 01	19	46	16.58	30.66	— 1	14.08	+ 0.08
44	"	α^2 Capricorni	— 13 20	7	59.79	14.11	— 1	14.32	— 0.23	
45	"	α Cygni	+ 44 43	20	34	47.44	61.75	— 1	14.31	+ 0.03
46	"	61 Cygni	+ 37 58	20	58	32.94	47.03	— 1	14.09	+ 0.24
47	"	ζ Cygni	+ 29 35	21	4	56.42	10.86	— 1	14.44	— 0.35
48	"	α Cephei	+ 61 55	21	13	34.03	48.06	— 1	14.03	+ 0.42
49	"	β Aquarii	— 6 16	21	21	56.99	11.42	— 1	14.43	— 0.39
	"	Polaris 1st wire		0	40	3.80				
	"	" 2d "		0	50	36.20				
	"	" mid "	+ 88 27	1	00	59.50				
	"	" 4th "		1	11	22.70				
	"	" 5th "		1	21	55.40				
50	"	Mean		1	0	50.50				
51	15	Polaris S.P.	+ 88 27	13	0	58.40				
52	"	Arcturus	+ 20 01	14	7	9.06	24.37	— 1	15.31	
53	"	β Libræ	— 8 48	15	07	11.74	27.15	— 1	15.41	— 0.05
54	"	α Cor. Bor.	+ 27 15	15	26	42.52	57.62	— 1	15.10	+ 0.32
55	"	α Serpentis	+ 6 56	15	35	11.06	26.33	— 1	15.27	— 0.16
56	"	β Scorpii	— 19 22	15	54	56.48	11.87	— 1	15.39	— 0.11
57	"	η Draconis	+ 61 53	16	20	37.80	53.08	— 1	15.28	+ 0.13
58	"	α Herculis	+ 14 35	17	6	9.54	24.75	— 1	15.21	+ 0.11
59	"	Polaris	+ 88 27	.1	0	56.40				
							The mid-wire bisecting the mark on the Bluehill.			
60	25	Polaris S.P.	+ 88 27	13	0	47.50				
61	"	Arcturus	+ 20 01	14	6	54.82	24.25	— 1	29.43	
62	"	α Cor. Bor.	+ 27 15	15	26	28.06	57.50	— 1	29.44	— 0.00
63	"	α Serpentis	+ 6 56	15	34	56.60	26.14	— 1	29.54	— 0.09

Abundant materials are furnished, by the preceding catalogue of transits, for deducing the state of the instrument, the direction of the meridian, and the true sidereal time. By means of these observations the rate of the clock is found to be

From the 10th to 15th July—1.16 From 15th to 25th July—1.41

s.	s.
—1.09	—1.43
—1.15	—1.43
—1.17	—
—1.12	—4.27
—	—
—5.69	Mean—1.42
—	
Mean—1.14	
—1.42	
—	
—2.56	
—	
—1.28	Mean from 10th to 25th July.

There are three methods in common use for determining the azimuth of the transit instrument. One of them employs the successive intervals between the upper and lower passage of Polaris; another compares the transit of two circum-polar stars, whose right ascensions vary about 12 hours; while the last depends upon the transits of high and low stars, including north and south stars. When the latter method is adopted, it is advisable to select stars whose difference of declination is at least equal to 40° . There are defects in all these methods. The first supposes the rate of the clock to be uniform during the 24 hours: this may not always be the case; but on account of the slow motion of the star, a considerable error in time would make but a small difference in the azimuth of the instrument. The second process depends upon the clock for a much shorter time; but it requires four observations, and the accidental error of these may exceed that produced in the first case by the clock. Lastly, the third mode requires only two observations, and depends on the clock only for a short time: but it supposes the tabular place of the star to be accurately known. A careful determination of the meridian must be based upon a combination of the three methods; when this is done, the instrument is adjusted with great exactness. This reduction has been made out of the transits in the table, and the azimuth of the instrument, when on the meridian mark, calculated from 18 different sets of stars, selected according to the known conditions of the problem, gives, as a mean result,

0".072 west of south, being the azimuth of the south end of the transit instrument.

The three successive transits of Polaris, observed on the 14th and 15th July, give,

0".11 west of south.

The final mean is 0".091 west of south.

The octagon apartments to the west of the transit instrument contain a Gauss Magnetometer, by which changes of magnetic declination are observed. They are built of wood, with copper and zinc nails; the walls rest on wooden posts; iron, stone, and all other substances known or suspected to exert magnetic influence, having been carefully excluded from every part of the building. *FD* is the direction of the astronomical meridian, and *DC* of the mean magnetic meridian. The three circles at *D* are the projections of so many wooden posts, which are bound firmly together at the top, and support the marble table on which a variation transit is placed. This instrument was made by Troughton and Simms, and is used in the magnetic observations. The larger interior circle represents the table; the chord at right angles to the magnetic meridian is the projection of the scale which is read in the observations: *C* is a Gauss Magnetometer, which is fitted up after the style of those at the Göttingen Observatory. The three circles at *C* are the feet of three posts, ten feet long, which unite at the top, eight feet above the floor, and give a stable point of suspension to the needle. The rectangle, enclosed within the feet, is the box which surrounds the magnet, and protects it from currents of air. Within the box may be seen this magnetized bar in the magnetic meridian, with a mirror firmly fixed to its south end to reflect the scale at *D* into the tube of the variation transit. The bar is suspended by a copper wire silvered, 0.011 of an inch in diameter, 5 feet 6 inches long, and, with its fixtures, weighs about 3 pounds.

From this arrangement it follows, that as the bar varies in its position, it must carry the mirror with it. The place of the mirror determines the mark on the scale which is reflected into the centre of the telescope; so that as the reflecting surface moves, the marks of the scale that are successively seen are read off, and employed to ascertain the motions of the bar. The scale at Cambridge is so divided, that the angular motion of the needle is read directly from it without any reduction. This novel mode of observing changes of magnetic declination is a great refinement upon the old methods, and has given an accuracy to the determination of this magnetic element, which has hitherto been considered attainable only in astronomy. For a more minute description of the Gauss instruments, and the directions to be obeyed in observing with them, the reader is referred to Taylor's Scientific Memoirs, Parts V. and VI.* It is necessary to omit in this place any further details, except such as are required in order to understand the remarks which follow. As all the observations embraced in this paper were made with the

* These instruments, directions, and the magnetic observatory, are fully described, with large folding plates, in vol. 3 of these Annals.—EDIT.

34 Professors Lovering and Bond on *Magnetic Observations*.

Gauss Magnetometer, I shall pass at once to them, and leave the description of the rest of the buildings, and the instruments placed in them, for the close.

The plan of magnetic observations, recommended by the Royal Society, and generally adopted at the magnetic observatories, prescribes that the declination magnetometer, and the horizontal and vertical force instruments, shall each be observed once every two hours during the 24 hours on every day of the year; that is to say, one of them is read 2^m 30' before each even hour; one at the even hour, and the third 2^m 30' after the even hour. One day in each month has been set apart for observations on the three instruments *at shorter intervals*. On these days, which are called term-days, the declination magnetometer is observed every five minutes, and the other instruments every 10 minutes, making four separate observations every 10 minutes, or 576 in the day. A short description of the instruments by which these observations are made is promised at the end of the paper. They have but recently been received and adjusted, and regular observations were made for the first time with them on the term-day of March, 1841.

The observations with the Gauss Magnetometer, which make the subject of this article, have been coincident in time with those taken with the declination magnetometer at the British stations, and are therefore comparable with them. The Report of the Royal Society on this great magnetic adventure, provides that the observations shall be made as closely as the nature of the instruments, with all the recent refinements of mechanical skill, added to the ingenious artifices of the observer, will permit; and the time is to be carefully noted, after having been determined by the assistance of astronomical instruments, so as to make the observations at the different stations, in a practical sense, simultaneous. Observations, according to this rigid system, were originally expected to extend over three years. When the materials, thus patiently gathered from so many remote sources, shall be collected, they will pass into the hands of competent persons, to furnish the elements for a complete theory of the earth's magnetism; and any attempt to pronounce, at this time, positively, on this great subject, would justly be regarded as premature. But it is equally certain, that there are secondary questions, of considerable intrinsic importance, which may be discussed now as well as at any time, the settlement of which will be facilitated by an occasional publication of portions of the regular magnetic observations. Much light may be thrown upon the general subject by a comparison of partial results; and parts of a well-concerted plan, to which no present objection appears, may prove useless in fact, and be either remodelled or superseded by other observations, which experience has shown will serve a better purpose. Moreover, the subject of terrestrial magnetism deeply engages the attention of the scientific world, and many, not actively engaged in the research, will be anxious to understand the progress of a scheme which promises

to shed light upon this complex problem. In this great country, it is highly important that something should be done to awaken the attention of individuals, and of the American nation, and to bespeak their bounty in favour of a scientific enterprise, which has received the highest patronage of European and other governments. The commercial prosperity, no less than the interests of pure science, will be affected by the spirit and liberality with which this bold project is sustained and conducted. It is hoped that the present publication may not prove useless for some of these purposes. A writer in the "*London Quarterly Review*," after speaking of the materials that will be collected at head-quarters, by three years' contribution, proceeds thus:—"Voluminous beyond all former precedent as the mass of data thus accumulated must of necessity be, we trust the whole will be printed (each nation and each department, of course, providing for the publication of its own). No consideration of economy should be allowed to interfere with the performance of this necessary duty, without which we look upon all that shall be done as virtually thrown away. Highly as we respect the illustrious body above mentioned, and applaud their selection of the individual into whose hands the results will in the first instance pass, yet their full, fair, and effectual discussion, can be secured by no other means than by inviting to it the collective reason of the age, and of all succeeding ones, and affording every one who may think proper to engage in the task, now or hereafter, ample opportunity to do so."*

It should be stated, for the information of those who have not watched the progress of the recent investigation into the laws of the earth's magnetism, that it originated with Baron Humboldt, and Professor Gauss, of Gottingen. In 1828, Baron Humboldt laid the foundation of the German Magnetic Association, by erecting a magnetic observatory at Berlin, which was imitated in various parts of Germany and Russia. In 1833, the attention of Gauss was called to the theoretical consideration of the earth's magnetism, but he found himself soon arrested by the want of accurate and extensive data. He instituted accordingly a magnetic observatory at Gottingen, and, having furnished it with new instruments, began observing in March, 1834. Co-operation was sought and obtained in various parts of the continent; a more intimate alliance was formed between the several points of observation; and, after various modifications in the time and mode of observing, the amount of which was, to make the observations more minute, and the number of terms smaller, one day in the months of February, May, August, and November, was selected, in which observations were to be made at every five minutes during the 24 hours. These are the present German magnetic term-days. In this advanced stage of the research, Humboldt addressed a letter to the President and Council of the Royal Society, soliciting the countenance and support of the British nation; and this appeal, after having been met in the most prompt

* *London Quarterly Review*, No. cxxxi, page 303.

and generous manner at home, was sent abroad to various Academies, and men of science in this country. The extent and important magnetic position of America were insisted upon in the appeal sent out to it, and it is to be wished that the example set by the magnetic observatory at Philadelphia, under the care of Professor Bache, and by the American Academy of Arts and Sciences in Boston, who have supplied the Cambridge Observatory with the requisite instruments, will be a motive to our government, and to individuals, to take measures in this matter worthy of their wealth and energy, as well as the eminent local advantages which they enjoy in regard to this scientific investigation.

The following table, from page 38 of the Report of the Royal Society, is published here for the information of those who have not seen the original.

Days of Commencement of the Terms of Simultaneous Observation, during the years 1840, 1841, and 1842.

Month.	1840.	1841.	1842.
January.....	Wednesday..22	Wednesday..20	Wednesday..19
February.....	Friday.....28	Friday.....26	Friday.....25
March.....	Wednesday..18	Wednesday..24	Wednesday..23
April.....	Wednesday..22	Wednesday..21	Wednesday..20
May.....	Friday.....29	Friday.....28	Friday.....27
June.....	Wednesday..24	Wednesday..23	Wednesday..22
July.....	Wednesday..22	Wednesday..21	Wednesday..20
August.....	Friday.....28	Friday.....27	Friday.....26
September.....	Wednesday..23	Wednesday..22	Wednesday..21
October.....	Wednesday..21	Wednesday..20	Wednesday..19
November.....	Friday.....27	Friday.....26	Friday.....25
December.....	Wednesday..23	Wednesday..22	Wednesday..21

The observations on the term-days, with the Gauss Magnetometer, were begun at the Cambridge University, in March, 1840, and no one has been omitted since that time.* The curves for only two of these days, the 29th of May and the 21st of October, are published; but the numbers which express the mean results for every five minutes during the 24 hours, on all the term-days, are placed in Table I, at the end of the paper. This mode is preferable in some respects to publishing the curves, as it enables any one who intends to make a comparison, to draw a curve for Cambridge, on the scale

* As the Report of the Royal Society, containing the details of their plan, had not been received when the observations commenced, two of the term-days were incorrectly taken, and the observations in April, besides being undertaken on the wrong day, were so imperfect, that they have been left out of the account entirely. Instead of March 18, March 27, and instead of July 22, July 24 were observed at Cambridge. This must be considered in the comparison of these days with observations made elsewhere.

used in observations made at other places. In our observations, the time was obtained accurately from the transit instrument and astronomical clock; and the scale which was reflected from the mirror at the southern extremity of the bar, and read off by the variation transit, easily allowed of being marked to $\frac{1}{8}$ of a minute. The experience of the observers has satisfied them that dependence may be placed upon each separate reading within that limit of error. But other considerations make it necessary to determine the position of the magnetometer, for any assigned time, by more than a single reading. As the bar, in passing from one angle of declination to another, is maintained always in a vibratory state, it is necessary to eliminate what is due to the oscillation from what belongs to an absolute change of declination. If the arc of vibration were constant, it would be eliminated by observing the limits of excursion of the magnetised bar, and taking the mean between them. But the natural tendency of the arc of vibration is to become shorter for every new excursion, and if the arc be of considerable length, this circumstance must be taken into account. As the decrease of arc must be nearly uniform for a few vibrations, this is done by noting the limits of three successive excursions, and the mean of two means thus obtained is the true position of the bar for the middle time. Thus, if a , b , c , are the readings, $\frac{1}{2}(\frac{1}{2}(a+b) + \frac{1}{2}(b+c))$ or $\frac{1}{4}(a+2b+c)$ gives the place of the magnetic meridian for the time when b was observed. If the arc of vibration is very small, this correction will be inappreciable, and the mean of two observations will suffice. But the declination itself, meanwhile, may vary by sudden and irregular movements, and then the process of observation and reduction becomes more intricate. Facts assure us that the magnetic meridian is subject to abrupt and lawless fluctuations, as well as uniform and progressive variations. The practical mischief of these disturbed motions is diminished, by the fact that they will most probably occur during periods of unusual perturbation; and although they must be kept in view when studying the laws of remarkable derangements of magnetic influence, their effect will be insensible in the regular and periodic changes.

A greater difficulty that affects particularly simultaneous observations is this. The precise moment of time to which the mean result corresponds may not be that for which the declination is sought; and the interpolation of the required times between the observed times is a matter of troublesome and uncertain calculation. This labour is prevented by an ingenious device of Gauss, in the way of observing. If two observations of a bar are made at an interval equal to the time of one vibration, the mean is the place for the intermediate moment. This is a proposition mathematically exact, if the change of declination can be regarded as uniform and the arc of vibration constant. It will, therefore, be practically true whenever no remarkable disturbances are apprehended, and the arc of vibration is small; or within the same limitations as the other

methods. If, now, the position of the magnetic meridian is desired for any definite moment, the first observation is made to precede this period by half the time of the bar's vibration, and the second to follow the period at the same distance. Thus, if t be the time of vibration, and T the time of mean observation, the actual observation must be made at $T - \frac{1}{2}t$ and $T + \frac{1}{2}t$. For greater accuracy, the final result is made to depend on several partial results, as will be seen by an illustration. The time of vibration of the Gauss Magnetometer used at Cambridge is about $54''$. This is divided into as many parts as separate observations can be conveniently taken during that time. It has, therefore, been divided into 6 intervals of $9''$ each, and a separate observation is made at each interval. This is done during two vibrations of the needle, or $1' 48''$. By taking the mean of every two observations which have an interval of $54''$, we have a partial result for the middle time, and these partial results are combined so as to give a final result for any time when the declination is required. If this time is $2^h 5'$ the first observation is made at $2^h 4' 6''$, and repeated at intervals of $9''$ till $2^h 5' 54''$. An example is given from the observations made June 26, at 0^h of Gottingen mean time.

H.	M.	S.	Readings of the Scale.	Partial Re- sults.	Times corres- ponding to Par- tial Results.	
23	59	6	108.750			
		15	9.000			
		24	9.500			
		33	110.000	109.312	$23\ 59' 33''$	
		42	10.125	9.062	42	
		51	10.000	9.250	51	
		0	9.875	9.375	00 0	
		9	9.125	9.312	9	
		18	9.000	9.375	18	
		27	8.750	9.375	27	
		36	8.500			
		45	8.750			
		54	108.875			

The Final Mean
of the 7 partial
results gives
109.294.

Mean 109.294 0 0

Thus it appears that each position of the needle is determined from 13 separate observations; and as each reading is to $\frac{1}{2}$ of a minute, the mean of all may be considered as within a smaller error of observation, and only subject to the exception, that the law of reduction is not rigorously exact when the change of declination, during $1' 48''$, is not uniform. If the arc of vibration be so large as to have a sensible decrease, the effect is cancelled when the readings extend through twice the time of vibration. On term days

this process is repeated every 5' during the 24 hours, so that 3,744 observations are made, which give 288 mean positions. This was the rule of the Observatory till June 26th, 1840, when a slight modification was introduced, which diminished the labour of observation and reduction, without compromising the accuracy of the result. Thus, the observation of June 26th, 3^h 40', P. M.,* which was the first one made in this way, stands thus: it should be remembered, that the column of figures to the right of the point are not tenths but eighths.

H.	M.	S.	Readings of the Scale.	Partial Re- sults.	Times corres- ponding to Par- tial Results.	
3	39	10	106.3			The Final Mean gives 105.854 as the number on the scale corresponding to the magne- tic meridian at 3 ^h 39' 59'' .5.
		19	105.7			
		28	105.3			
		37	105.2	106.	39' 37''	
		46	105.1	105.937	46	
		55	105.2	105.812	55	
		4	105.5	105.875	40 4	
		13	106.0	105.750	13	
		22	106.2	105.750	22	
		31	106.4			
		40	106.3			
		49	106.2			
Means.			105.854	105.854	39' 59'' .5	

By this method, which is the same in principle as the other, only 12 observations are made, and the mean of them is the same as the mean of the partial results, so that the latter column in the table is unnecessary, and a great part of the labour of reduction is saved. The number 12 is a convenient divisor, and after the whole minutes are found the decimals are taken out of a table calculated for this purpose, and embracing every case that can occur. Look in the vertical column at the right or left for the whole numbers of the

* Gottingen mean time is to be understood wherever it is not otherwise stated.

remainder, after dividing by 12, and in the top or bottom line for the eights, and in the corresponding square is the decimal value of the remainder.

	0	1	2	3	4	5	6	7	
0	000	010	021	031	042	052	062	073	0
1	083	094	104	115	125	135	146	156	1
2	167	177	187	198	208	209	229	240	2
3	250	260	271	281	292	302	312	323	3
4	333	344	354	365	375	385	396	406	4
5	417	427	437	448	458	469	479	490	5
6	500	510	521	531	542	552	562	573	6
7	583	594	604	615	625	635	646	656	7
8	667	677	687	698	708	719	729	740	8
9	750	760	771	781	792	802	812	823	9
10	833	844	854	865	875	885	896	906	10
11	917	927	837	948	758	969	979	990	11
	0	1	2	3	4	5	6	7	

It must be observed, that the mean result obtained above corresponds to $3^h 39'' 59''.5$, and not $3^h 40'$. But the difference of half a second comes within the limits of unavoidable errors of observation, and is of no weight in deciding on the comparative merits of the two methods, each of which depends on a knowledge of the time of vibration of the bar. But this time changes slightly from one period to another, and although always assumed to be $54''$, it is strictly $53''.4$, on the average, and oscillates about this mean value.* Whenever observations have been made with the Gauss Magnetometer, since June 26th, 1840, it has been the rule of the Observatory, recommended by its superior simplicity, and freedom from all practical objections, to take 12 readings at

* The mean of 28 vibrations in April was $53''.05$; of 21 in July, $53''.38$; of 22 in September, $53''.45$; of 12 in October, 53.65 .

intervals of 9', commencing 50'' before the real time, and to consider the mean of them as the final determination of position for that moment. Neither the Gauss method, nor that of Cambridge, which is based on it, is practicable when the bar is agitated by unusual magnetic influences, as in seasons of violent disturbance, in consequence of the great extent of its motion. In such emergencies, the extreme of every excursion is recorded, so long as this perturbation continues, and an approximate time is obtained as exact as circumstances allow. The reduction is then made by this formula, $\frac{1}{4}(a + 2b + c)$, which has been already explained. After the mean results for every five minutes, during the 24 hours of a day, are obtained by any of these processes, they are used as the data for projecting a diurnal curve of magnetic declination. Two lines are drawn upon a sheet of paper, at right angles to each other, and assumed as the axes of rectangular co-ordinates. One of them is divided into 24 equal intervals, each of which is subdivided into smaller parts, according to the scale of the chart. The other line is also divided in portions corresponding to degrees and minutes of arc. Any point that is most convenient may be selected as the origin of the co-ordinates, and, by considering the time as ordinate, and the result of observation annexed to it as abscissa, we obtain as many points of a daily curve as there are mean results of observation. In ordinary term-days the number is 288. When so many points are fixed upon the sheet, they are connected by straight lines or curves of the simplest curvature. From the details published in regard to the principle of observing, it may be inferred how closely these curves will represent the actual magnetic changes for the day. It cannot be denied that disturbances may happen, of less amount than the minimum quantity of observation, or at less periods than 5', which will elude the vigilance and refinements of the present state of magnetic science. It has been noticed on more than one occasion that the bar has been instantaneously checked in the midst of a vibration, and forced to retrace its steps by a long sweep in the opposite direction. The lines which are now drawn straight, or in the most natural curve from one fixed point to another on the sheet, might, if they were sensible of the shorter and more rapid magnetic impulses, change their curvature several times during the passage.

Plates II. III. IV. and V. represent the diurnal curves of magnetic declination for the days given on the plates; and we are first to consider, from an attention to them, as well as to the figures which describe the other days at the end of the communication, whether the fact of a regular cycle of variations in the declination during the 24 hours is confirmed by these observations. The theory appears now to be well established, that the elements of terrestrial magnetism are subject to daily, monthly, yearly, and secular perturbations, similar to the periodical and secular variations which are known in astronomy. But in the astronomical problem, no derange-

ment occurs whose cause is not looked for and generally found in the uniform operation of the simple law of gravity in its direct or reflected action upon the various members of the solar system. The singular fact of Encke's comet, which experiences a delay which has been attributed to a resisting medium, may be regarded as a solitary exception to the general truth. The laws of motion among the heavenly bodies are so few and clear, that the character of the disturbance will generally indicate something in regard to the cause which produces it. But the elements of the earth's magnetism are exposed to abrupt and violent fluctuations, which, so far as the circumstances are known, acknowledge no periods, and, although perhaps capable of being explained by many conceivable causes, which are in constant operation, and, therefore, at the disposal of the philosopher, they cannot be distinctly brought home to any single one, and are at present regarded as inexplicable. These magnetic hurricanes, as they have been fancifully called, are often exhibited during auroral appearances, but many of them, so far as has been observed, are not coincident in time with this or any other class of natural phenomena. Now, every observed position of the needle, for a given moment, is beset with all these regular and irregular variations; which must be carefully eliminated, by multiplying the number, and shifting the exposure of the observations, before we can be assured what is the exact and absolute value of the element for that time. And when we are seeking the amount of any particular order of perturbations, we must proceed as in the astronomical case, by selecting, as far as may be, times for observation, when the disturbance in question is a maximum, and all others are of minimum value. The practice of the observer will supply many artifices of this sort, for eluding or grappling with difficulties, which appear, at first sight, insurmountable. It is obvious, that when the object is to ascertain the steady and periodical variations of the meridian, we should exclude from the comparison those days whose serenity is affected by what Humboldt has denominated magnetic storms, just as we should pass over days of violent winds and tempests, in deducing the gradual rise and fall of temperature during the 24 hours. No attempt should be made to frame an hypothesis, or even to hazard a conjecture in regard even to the variations of the shortest period from one year's observations, however unremitted they may have been; but these observations may be of use in confirming a theory long entertained and well established by facts noticed in other places.

The observations of Graham, in 1772, which resulted in the detection of the diurnal variation of the magnetic meridian at London, have been repeated since in various parts of the world with increased delicacy and skill, and with the same general result which is briefly described. The magnetized bar, free to place itself in the magnetic meridian, does not remain in one fixed position during the day; but sometime in the morning, between six and eight o'clock, as

the average statement, it starts in a westerly direction, and moves that way till between one and three in the afternoon; then it begins to retrace its steps back to the east again. These points of maximum and minimum declination are formed in every diurnal curve, and at nearly the same hour. We shall hereafter see what the limits of the time are. There are two ways in which the bar regains its first position. In some places, as Paris for example, it arrives at its greatest eastern elongation again between eight and eleven o'clock in the evening, and then remains stationary till the time of morning excursion has come round once more. In other places, as at Cambridge, it travels eastward till evening, and then goes back to form a secondary point of maximum westerly deviation about three o'clock, A. M.; after which it passes eastward, and recovers at eight o'clock the place it occupied 24 hours before. In certain cases, especially in northern latitudes, even when the secondary maximum and minimum are not formed, the bar does not remain stationary during the night, but occupies nearly all the time from three P. M. to eight A. M. in returning through the space it has just passed over in seven hours. Again, the arc traversed by the bar in its daily excursions, varies perceptibly from one day to another; but the approximate law is, that in the six months from the vernal to the autumnal equinox, its value is between $13'$ and $15'$; and, in the remaining six months, the mean of the daily arc is between $8'$ and $10'$. But there may be single days when it amounts to $25'$, and others when it is as small as $6'$.

Gauss thinks that eight A. M. and 1 P. M., of mean solar time, are never far from the periods of daily minimum and maximum declination in Gottingen, and that part of the globe. It appears, from a report in regard to the magnetic state of the Russian empire, for 1837,* that, at St. Petersburg, the greatest westerly position of the north end of the magnetic meridian is near two o'clock P. M., and the opposite position is at eight in the morning, with the exception of November, December, and January, when it occurs later. This is easily explained, by the high latitude of the place, when we come to consider the dependence of this daily motion on the sun. Since the declination is easterly in some parts of Russia, it follows that the maximum declination there is in the morning, and the minimum in the afternoon. As the report in question has been published with great care, we extract a table of the monthly

* *Annuaire Magnetique et Météorologique du Corps de Ingénieurs des Mines de Russie.* Année 1837. St. Petersburg, 1839.

means of the arc of daily excursion, to shew how near they correspond to the more extended means, which we have mentioned above.

St. Petersburg, 1837.

April	16.2	October	6.7
May	15.1	November	3.7
June	16.5	December	1.9
July	13.6	January	3.2
August	11.9	February	5.8
September	9.9	March	11.0
	83.2		32.3
	13.86		5.4

We have no occasion, in this place, to remark on the cause of this great inequality of arc in the different months, or on the times when the maximum and minimum occur. We only wish it to be observed, that the daily curve, so perceptible in other places, and so marked by its general uniformity of appearance, is clearly seen in the Cambridge observations. We have here referred to the plates, which represent some of these diurnal curves; but Table II., at the end, will display, in a condensed form, the results of 12 months' observation on this point. As the values for some of the months were deduced from scanty observations, they cannot be brought into a fair comparison with more comprehensive means. If it shall appear that the times of maximum and minimum declination are embraced within the limits of a few hours, it is extremely important that observations should be made during those periods every day in the year, to determine the precise moment when they occur each day, and the arc of excursion. Means drawn from such abundant data might lead to a satisfactory solution of the daily changes, and not leave them, as at present, to probable conjecture.

It appears, from observations on various parts of our planet, that periodical changes of days, months, years, and longer duration, are affecting its magnetic equilibrium; all of which come, with few exceptions, under the same general expressions, so much so as to leave no doubt that they have a common origin. Hence it might be expected, that if the corresponding curves of magnetic declination were drawn, for the same solar time, in different places, they would conform to each other, and exhibit a kind of parallelism. But in addition to the orderly variations, there are other perturbations of a sudden and irregular nature; and it becomes an object of extreme interest to inquire how far these are local and accidental, and to what extent they must be regarded as general, and proceeding from some grand central force. Such an examination was contemplated,

as among the good results of the simultaneous observations made in Germany, Russia, Italy, &c., an account of which may be found in Taylor's "Scientific Memoirs," and "The Russian Magnetic Annual," to which I have already referred; and a possible answer to an intricate question was held out as an inducement to engage in the magnetic crusade. The expected comparison exhibits surprising coincidences, in most of the irregular movements of the magnetometer, as if the cause were co-extensive with the range of magnetic posts. Hitherto our country, and, indeed, this whole western continent, has not been represented in this congress of nations for a scientific object; having been destitute of the means of contributing its portion to the general levy which has been made upon them. But the term-day observations at Toronto, U.C., Philadelphia, and Cambridge, will furnish materials for doing this now. Plate III. exhibits the diurnal curves of the October term-day, for the magnetic observations of Toronto and Cambridge. The declination of the meridian was very considerably deranged by perturbations during the first 12 hours of the magnetic day (which always begins at 10 P.M. of *Gott. M. T.*), and affords, therefore, a favourable opportunity of discovering the extent of extraordinary disturbances. Now a glance at the plate betrays wonderful concert in the motions of the two remote bars; almost every digression of the bar, at Cambridge, and every change of curvature in the magnetic curve of that place, has something corresponding to it in the curve of Toronto. There are a few singular exceptions; and, in a word, the bar at Toronto seems to have been more agitated, and to have made greater excursions. But these discrepancies throw no doubt on the subject; for they are just such as must be expected to occur in results which depend upon complex and multiform agents. A general, and not a mathematical agreement, is all that can be expected. The times which mark the limits of the eastern and western excursions, in this fluctuating motion of the bars, agree with great precision, except in two or three instances, where the Cambridge curve lags behind five minutes or less. The parallelism appears at once, from considering that the two curves, starting from points 12 minutes apart, and making several digressions, the same way, of greater amount than the original arc of separation, do not cross each other except once; and this solitary instance will hardly be regarded as a transgression of the rule, since it arose simply from one bar being more affected by a particular wave of the magnetic tide than the other bar. On Plate II. is described the diurnal curve for the May term-day. This, too, is singularly disturbed from 11 P. M., *Gott. M. T.*, till 10 o'clock of the next morning. We have had an opportunity of comparing this with the similar curve observed by Professor Bache, at the Girard College, in Philadelphia, and here again the instances of agreement make a stronger impression than the rare cases of discrepance. The correspondences are frequent and imposing; while the points which exhibit so much coincidence

are few, and of less importance. The agreement as to time, is, in almost every place, precise; the disagreement is confined chiefly to the extent or the existence of the motion. At $12\frac{1}{2}$, $7\frac{1}{4}$, $9\frac{1}{2}$ o'clock, Gott. M. T., some excursions appear in the Philadelphia curve, which have little or no counterpart in the Cambridge curve. They are deserving of notice, because they are more considerable than any others of a local character which have been noticed, and yet they do not, any of them, exceed $5'$ of arc. But the remarkable excursions in Plate II., at *a, b, c, d, e, f*, are all faithfully represented on the Philadelphia curve. Only the upper branches, which belong to the easterly motion, are exceeded by the corresponding simultaneous movement at Philadelphia, while the grand excursions, at the bottom of the plate, which are due to the westerly motion, or increase of declination, go beyond their parallel passages in the Philadelphia curve. For instance, the branches, *b, c, d, e, f*, extend respectively $10'$, $12'$, $11'$, $5'$, $6'$, farther in the Philadelphia than the Cambridge curve. The reverse happens at *g, h, i*, where the Cambridge branches go $13'$, $30'$, $14\frac{1}{2}'$, farther west than the analagous ones at Philadelphia. The close coincidence of the times when the direction of any great motion changes, observed at Toronto, Philadelphia, and Cambridge, makes it probable that the longitudes of the three places are known to a close approximation, or, at least, that the difference of longitude between these places are not much in fault; for, if they were, it would, by its effect upon the regulation of the clocks, prevent a coincidence of magnetic disturbances which actually existed, and make the appearances different from what we observe; unless we can suppose, what seems extremely improbable, that the constant error of the time should by chance balance precisely the actual want of coincidence in the arrival of the magnetic impulse at each place so as to deceive us in the final result. This, however, is a subject which requires to be pursued longer. The observations contemplated in coming years will, it is believed, furnish the materials for a comprehensive study of this problem, and enable us to determine, with more confidence than can be reposed in the comparison of places not widely removed from each other, the extent and the laws of what are now classed among the irregular perturbations of the magnetic equilibrium. All attempts at induction now must be considered as subordinate to the final discussion; but they have their purpose in indicating from time to time the direction to which the attention of observers should be particularly turned. To this end an arrangement, supplementary to the large plan, was completed with Lieutenant Riddell, for making simultaneous observations on the declination magnetometer at Toronto and Cambridge, at intervals of *two minutes* from $0^h 45'$ to $1^h 45'$ P.M. Gott. M.T. Such observations were accordingly taken at Toronto for the assigned hour every day (Sundays excepted), from October 23d to November 19th, inclusive. Similar observations were made in Cambridge at the same time. Professor

Bache would gladly have been a partner to this project, but an accident prevented him from being informed of it in season. Another agreement was formed to observe *every vibration* of the bar during one hour, from 9 to 10 P.M., Gott. M.T., for one week, commencing with the 4th of January, 1841. Fig. 2d of plate III. exhibits the hourly curve for the 28th of October; the upper one being the Cambridge curve and the lower one the Toronto curve. Here it will be noticed that the changes of flexure in the two curves are generally coincident in time, and when they vary, the maximum difference does not exceed the interval between two successive observations.* The hour selected for the January observations appears, on the whole, unfavourable for drawing conclusions in regard to the concurrence of abrupt changes of the magnetic state of the earth, as it is the period when the bar is most quiet. It is of advantage, however, to know that the periods of repose are independent of longitude, and the same absolutely for different places; an inference which the observations certainly authorize us to make. We shall have occasion, hereafter, to remark on the degree to which observations made with the Gauss magnetometer and Lloyd's declination instrument are comparable.

Whatever interpretation may be given to the anomalous changes to which the declination of the magnetic meridian is subject, there can be little hesitation in admitting, that the regular and periodical ones, having their expression in functions of solar time, are dependent upon the sun's influence as an exciting and sustaining cause. The theory which we may adopt as to the nature of the earth's magnetism, does not essentially affect this statement. If magnetism, as an independent property, exists in particles of the earth's mass, it may have its equilibrium disturbed by temperature, as heat is known to affect the state of ordinary steel magnets; and as in the passage of the sun through his daily and yearly path, parts of the earth's surface are heated to unequal degrees, that change of the magnetic fluid may be induced, which shall result in periodical alterations of the magnetic meridian. But a strong body of evidence can now be summoned, to prove that magnetism has no existence as an independent fluid or property of matter; but that polarity is one of the phases of the electrical fluid. The magnetic character of currents, and the facility with which they are excited by motion and differences of temperature, are now well approved facts. The earth's rotation, the sun's heat, volcanos, and the great eastern and western metallic ranges, furnish all the materials and machinery

* While this paper was passing through the press, we received an account of the proceedings of the Irish Academy at a recent meeting, containing some remarks by Professor Lloyd, on contemporaneous observations at short intervals, by himself and Professor Bache, on the small variations of magnetic declination. The result of the comparison has convinced him, that such changes do not occur simultaneously at places so far removed from each other as Dublin and Philadelphia. Therefore, they will not, as was suggested, furnish a safe method of deducing differences of longitude.

that are needed for making the planet a grand electro-magnet. Aside from the magnetic phenomena, the existence of the earth's currents is thought to have been shown by direct experiment. If we do not admit them as the prime source of terrestrial magnetism, they offer themselves as sufficient and satisfactory causes of the observed fluctuations about the average state. The belief in the earth's currents seems unavoidable; and if any proportion subsists between their magnetic energy and those of artificial electro-magnets, the fund of magnetism which they supply must be ample to explain all the chief facts of the earth's magnetism. But if we adopt Gauss's estimate, that the whole magnetic power of the earth is equivalent, on the average, to 7.8 steel magnets of one pound weight, magnetized to saturation for every cubic metre, there may be some difficulty in conceiving of sufficient iron ore in the earth to furnish the depository of such a magnetic force according to the common motion of steel magnets. Besides, there may be a question whether it is philosophical to look round for any such new principle, when it is not needed to explain the facts. If currents are finally adopted, not as the auxiliary, but the main and standing cause of terrestrial magnetism, the unavoidable fluctuations and occasional overflowings with the daily ebb and flow, to which from their nature and remote source they must be subject, afford a liberal explanation of the regular and irregular perturbations in the declination of the meridian.

In so complex a subject, no attempt could be made at present to explain the *modus operandi* by which the sun's rays affect the position of a magnetised bar. It is much to have the dependence of these changes upon solar time clearly established. The moments at which the maximum and minimum points of the diurnal curves occur, oscillate between certain limits; it is important, therefore, to have the diurnal curves for places removed many hours from each other in longitude, in order that a difference of time, depending upon the constant difference of longitude, may not be masked under the variation to which the time is subject at the same place. Now, the observatories of Gottingen and Cambridge differ in longitude $5^{\text{h}} 24' 16''$; it is plain, then, that if there be such a relation to the solar time of each place, it must appear even in the curves of a single day. And so it is found, by an examination of the Cambridge times, and those published by Gauss, for Gottingen and contiguous places, that a difference, equal to the difference of longitude, exists in the mean times of the greatest eastern and western declination of the magnetic meridian. The extremes occur at nearly the same solar hours at each place, so that when we are observing our minimum in the morning, the observer, in those eastern longitudes, is taking note of the western excursion of his bar; for at both places the interval from minimum to maximum is between five and six hours. The rest of the 24 hours is expended in the return of the bar back towards the morning position, except in those cases where

a secondary maximum and minimum occur; and if this unequal time of passage over the same arc cannot be explained, it is analogous to what is known to be true of the rise and fall of the temperature on the earth. The heating requires a shorter time than the cooling process; and whatever depends on the heating and cooling process, will, of course, be subject to the same inequality. And, in regard to all the magnetic changes which are attributed to the solar influence, it is to be observed, that no more exact uniformity is to be expected in them than in the legitimate and acknowledged influences of the sun; that is to say, the earth's temperature.

Yet, further, supplementary to the plan recommended by the Royal Society, a series of observations have been undertaken, at Cambridge, with the express object of throwing additional light on this interesting inquiry. As early as May, 1840, the observations took a wider range, so as to furnish a diurnal curve, not only for the term-day, but also for several other days in the same part of the month. The following table shews the days over which the observations were extended, including the prescribed term-days:—

		Term-day.	Extra Days.
1840.	May.	May 29	May 21, 22, 23.
	June.	June 24	June 21, 22, 23, 25, 26, 27, 28, 29, 30. July 1.
	July.		July 24.
	August.	August 28	August 29, 30, 31. Sept. 1.
	September.	September 23	September 21, 22, 24, 25.
	October.	October . . 21	October 20, 22, 23, 24.
	November.	November 27	November 23, 24.
	December.	December 23	December 21, 29.
1841.	January.	January . . 20	January 22, 25.
	February.	February . . 26	February 24. March 1.

(To be continued in the next Number.)

Note on the Decomposition of Bioxalate of Ammonia by Heat, and the Products which result therefrom. By M. BALLARD, Professor at the Faculty of Sciences, Paris.* Commissioners: M. M. THENARD, CHEVREUL, DUMAS.

The principal phenomena which accompany the decomposition of the oxalate of ammonia by fire, are already well known to chemists. M. Dumas, in 1830, published a work of high interest on this subject; but although the discovery, so remarkable, of oxamide, had made it evident how advantageous for science the attentive

* Comptes Rendus, Aout 16th, 1841.

examination of the action of heat on the ammoniacal salts with organic acids would be, very few works, however, have been employed in this direction; and if the compounds of which oxamide is the type, be successively increased, it is in these circumstances entirely that the study of the action of fire on the correspondent ammoniacal salts have become the acquisitions of science.

This too much neglected study appears, however, likely to contain that which was anticipated by the discovery of oxamide, and probably to become a fruitful source of new compounds of azote. M. Vœhler, very recently, has determined that the action of heat on the mellitate of ammoniac, and the existence of paramide and of the euchronic acid, has been the result of his interesting labours. I have been occupied myself for some time on the action of fire on the bioxalate of ammonia. This study has permitted me to make some new observations, and to prove the production of a particular acid, which presents an example, not in the least doubtful, of the existence of amide acids.

When the crystallized bioxalate of ammonia is exposed to the action of heat obtained by the employment of an oil bath, the salt touches on or undergoes the aqueous fusion, according as it retains, in crystallizing, a greater or less quantity of water; loses this water, and commences laying the foundation of the igneous fusion, and becomes decomposed when the thermometer, plunged into the bath, indicates a temperature from 220 to 223°. This fusion is at first only observed on those portions of the salt neighbouring to the partitions of the cucurbit, and it is conceived that the heat, being only propagated slowly in this bad conducting mass, will cause much time to elapse in attaining the central parts, and thus only raise the different points to very unequal temperatures; but if, by the superior opening of the cucurbit, which it is necessary to have care to choose of a tubulated form, we effect the mixture of that portion of the salt which is already melted with that which has not yet attained that state; the whole body moistens, and soon forms a mass, at first liquid, afterwards clammy, which bloats up or puffs strongly after an abundant gaseous disengagement, carried on in its interior. This gaseous disengagement, which acts constantly in a mixture of divers portions, spreads the heat equally, and maintains the uniform decomposition in all the matter, which, without this circumstance, would be far from being the case.

The gases which are disengaged are those which accompany the decomposition of the oxalic acid by fire; they consist of a mixture of oxyde of carbon and of carbonic acid, in which the latter predominates. There becomes condensed in the receiver a remarkable quantity of formic acid; in the receiver and in the neck of the cucurbit will be found a small proportion of oxamide, very white.

As soon as the bubbling or puffing up manifests itself, the liquor which runs into the recipient is acid, and the addition of potash does not eliminate any ammonia; but if, when the gaseous disen-

gagement has ceased, we continue the action of heat, the mode of the decomposition is changed, and the results change also in their nature. There is then formed, amongst other products, a yellow matter of a bitter savour; cyanhydrate and carbonate of ammonia are disengaged, and the liquor which condenses in the tube causes an effervescence on falling into the acid liquor which the receiver contains. On this appearance becoming manifest, it is necessary to put an end to the operation, for the heat will decompose by degrees the whole mass, and thus destroy the compounds which it has contributed to produce.

The fixed matter which remains in the cucurbit presents itself under the form of a light and porous mass, of a very pale yellow colour, if the heat has been conveniently managed, but of a reddish tint, if the action of this agent has been suffered to proceed too far. Cold water, in acting on this mass, leaves an insoluble portion of coloured oxamide, and dissolves a matter which is no longer bioxalate of ammonia; for the aqueous solution, whose reaction is acid, neutralized by the ammonia, does not mix with these extended solutions of salts of lime, and of baryte, but produces, with these concentrated solutions, a crystalline precipitate, soluble in boiling water; this, upon cooling, leaves deposited very distinct crystals, often coloured yellow, but which may be obtained pure and colourless by being treated with animal carbon and a new crystallization.

These crystals contain, in the state of salt, a particular acid, which I shall designate, in the course of this work, under the name of *oxamic acid*.

It sometimes happens, and that oftener than not, that some portions of bioxalate having escaped the decomposition, the precipitate contains a certain proportion of oxalate of lime; but the insolubility of this latter compound in cold water permits the separation of the oxamate of baryte, and enables us to obtain, in a state of purity, this saline compound, which may afterwards serve in the preparation of many others of the same class.

This salt of baryte, on which I have directed my experimental researches, produces a gaseous disengagement of ammonia, when heated by alkali; the liquor then contains nothing more than an alkaline oxalate. On the other hand, this salt, when submitted to the action of concentrated sulphuric acid, leaves disengaged equal volumes of carbonic acid and oxyde of carbon, and the acid, which remains colourless, retains ammonia in combination.

This double mode of composition, shews thus: that the acid of this salt is susceptible, like oxamyde, of being transformed, with the concurrence of the elements of water and ammonia, and of oxalic acid, or in products of its decomposition, and permits the deduction of the composition of the oxamic acid from numerous experiments; for it is only a small number of formula which agrees with the formation of these products, and the determination of the

equivalent of the acid will suffice, with rigour, to make a choice from amongst them.

Now one gramme of the oxymate of baryte gives 0.636 of the sulphate of baryte; and on the other hand, this salt, which contains water of crystallisation, leaves it completely disengaged when we submit it to a current of dry air heated to a temperature of 150° , which commences its decomposition, and to disengage the traces of ammonia.

2 grammes thus heated have lost : water, 0.309.
1.867 „ „ have lost : water, 0.267.

It results, from these experiments, that the salt of baryte contains 3 equivalents of water, and that the chemical equivalent of anhydrous oxamic acid is 998, a number from which may be deduced, for the composition of oxamic acid, the following formula :— $C^4 O^6 H^4 Az^2$.

The number 1.002, which gives the calculation for the atomic weight of such an acid, approaches very nearly in effect to 998, determined by direct experiment.

The analysis of the salt of baryte confirms this composition :—1 gramme of oxamate of baryte, analysed by the oxyde of copper, produced 0.394 of carbonic acid, and 0.279 of water.

One gramme of the same salt furnished 67 cubic centimetres of azote, measured at the temperature of 14° , and under the pressure $0^m.755$. By diminishing the water produced, that which the salt contained in the state of water of crystallisation, and adding to the carbonic acid obtained, the portion which the baryte should retain, by admitting that it remains quite entire in the state of carbonate, we find, for the composition of oxamic acid :—

	Formula.	Calculation.	Found.
C^4	300	29.94.....	30.08
O^6	500	49.90.....	49.26
H^4	24.95	2.49.....	3.33
Az^2	177.03	17.67.....	17.33
	1001.98	100.00	100.00

The analysis of other oxamates confirms that of the oxamate of baryte.

If we boil this compound with a proportionate quantity of sulphate of ammonia, in solution, we obtain, by evaporation and cooling down, a salt which crystallises in small prisms grouped in stars, and which contains only the equivalent of water which is indispensable to the constitution of ammoniacal salts.

In fact, an undetermined quantity of this salt, burnt by the oxyde of copper, has given carbonic acid and azote, sensibly, in the proportion of 2 volumes of the former for 1 volume of the latter; and 0.500 of this salt have furnished :—carbonic acid, 0.407; water, 0.260.

We deduce from these numbers, for the analysis of ammoniacal salt :

	Formula.	Calculation.	Found.
C ⁴	300	22.5	22.3
H ¹²	75	5.6	5.8
Az ⁴	354	26.5	26.5
O ⁶	600	45.4	45.4
	1329	100.0	100.0

A solution of oxamate of ammonia and oxamate of baryte, treated by nitrate of silver, furnished an abundant gelatinous mass (magma), demi-transparent, and which soon becomes opaque. This precipitate is completely dissolved in the liquor when we raise the temperature, and this, by its cooling down, leaves deposited silky crystalline needles, white, but which recover the metallic silver at their surface, and also become blackened when they are exposed to the action of light. A temperature of 150° produces the same effect amongst them, but does not eliminate any water.

0.982 of this salt, decomposed by the fire, leaves 0.540 of metallic silver. We deduce from it, for the equivalent of the acid, 1.005 ; the calculation gives 1.002.

1.104 of the same salt have produced :—carbonic acid, 0.495 ; water, 0.116.

0.639 of the same compound have given :—azote, 39 cubic centimetres, temperature 17°, barometer 0^m,7645, whence we deduce for the composition of the acid contained in the salt of silver :—

	Formula.	Calculation.	Found.
C ⁴	300	29.9	29.9
O ⁵	500	49.8	49.9
H ⁴	24.95	2.5	2.8
Az ²	177.03	17.7	17.4
	1001.98	100.0	100.0

Each of these salts, of which I have already spoken, will serve to isolate the oxamic acid.

That of baryta, treated cold, by a proportionate quantity of sulphuric acid, much diluted with water, gives, by evaporation of the liquid executed at the ordinary temperature, free oxamic acid.

A saturated solution of oxamate of ammonia, hot, treated by a proportionate quantity of sulphuric acid, and suddenly cooled, also leaves deposited oxamic acid, under the form of a white powder, in a small degree soluble in cold water.

In fine, the oxamate of silver, on which we cause to pass a current of dry chlorhydric acid, undergoes, by a slight elevation of temperature, a decomposition, which is effected with an energetic disengagement of heat, and with the production of chlorure of silver

and free oxamic acid. In driving away the excess of chlorhydric acid by a current of dry air, and by treating the mixture of chloride of silver and oxamic acid by alcohol absolutely boiling, we obtain, by the evaporation of this liquid, oxamic acid, under the form of a granulous powder, colourless when we moderate the action of the chlorhydric acid on the salt of silver, and avoid too great an elevation of temperature; but slightly coloured in yellow, if this precaution has been neglected.

0.335 of this acid, burnt by the oxyde of copper, have furnished 0.325 of carbonic acid, and 0.120 of water.

0.318 of this acid have produced azote 46° , temperature 18° , pressure 765,2; which gives, as the analysis of the free acid and its formula, calculated on the supposition that it contains 1 equivalent of water:—

	Formula.	Calculated.	Proved.
C ⁴	300.....	26.9.....	26.1
O ⁶	600.....	53.8.....	53,4
H ⁶	37.....	3.4.....	3.9
Az ²	177.....	15.9.....	16.6
	1114	100.0	100.0

The analysis of the oxamates and of the free oxamic acid fully agree, leaving not the least doubt on the composition of this compound. Anhydrous and such as it exists in the dry oxamates; the oxamic acid contains the elements of bi-oxalate of ammoniac, also anhydrous, less those of one equivalent of water. It is with bi-oxalate of ammonia that this oxamide is neuter to the oxalate. In its anhydrous state, oxamic acid will thus be isomeric with the alloxane of M. M. Liebig and Töhler. The isolated acid of these salts contains one equivalent of water, and contains the elements of bi-oxalate of anhydrous ammonia.

Like the amides to which they assimilate, oxamic acid retakes the elements of this equivalent of water, and thus reproduces the compound which has given birth to it. This return to the primitive state is brought about by the action of water, aided by the concurrence of heat. But whilst a temperature of 200° is necessary to transform the oxamide into the oxalate of ammonia, by the action of water alone, this liquid only requires a temperature of 100° to produce on the oxamic acid an analogous decomposition. Likewise, when we attempt to isolate the oxamic acid in decomposing, in a hot state, by sulphuric acid, a solution of oxamate of baryte, and evaporate the liquid at an elevated temperature, we only obtain bi-oxalate of ammonia. The decomposition is complete, and thus I am perfectly assured by analysis of the product of this reaction.

Among the azotic acids known to chemists, there is one to which the oxamic acid nearly approaches by its nature, and the mode of

the decomposition which it undergoes on being brought into contact with water; this is the oxaluric acid, which contains the elements of two equivalents of oxalic acid and one equivalent of urea, less those of one equivalent of water, which becomes transformed into oxalic acid and oxalate of urea by the action of water, at its boiling temperature. This analogy of nature, and this similarity in the mode of decomposition, coinciding further with a great resemblance in their properties, there exists, in fact, between the oxaluric acid and the oxalates, oxamic acid and the oxamates, the most strict connexions.

The oxamic acid, which is formed during the decomposition of bi-oxalate of ammonia by fire, may be obtained, likewise, by submitting to the action of this agent the neuter oxalate of this base. In his work on oxamide, M. Dumas has remarked that the action of heat on the neuter oxalate occurred by disengaging at first the ammonia, and that it was thus on a salt, with excess of acid, that the decomposition which gives rise to the oxamide becomes realised further on. It was from thence natural to research, among these products, the presence of oxamic acid, which I have found again in the fixed residue of this reaction, determined by a well-managed heat.

This mode of the action of fire on the oxalate of ammonia differs a little from that which this agent exercises on the mellitate of this base. After the recent researches of M. Töhler, the mellitate of ammonia, exposed to the action of a heat of 150°, at first loses ammonia, and is transformed into mellitate acid, which in its turn undergoes a decomposition, at the end of which it produces paramide and euchronic acid. But neither one nor the other of these compounds correspond to the oxamic acid which I have described. This acid is bi-oxalate of ammonia, less the elements of one equivalent of water; paramide, on the contrary, is bi-mellitate of ammonia, less the element of two equivalents of water; in fine, euchronic acid will be trimellitate, less the elements of three equivalents of this liquid.

The knowledge of the oxamic acid appears to me to be susceptible of fixing the opinions of chemists on the true nature of some ethereal compounds, of a constitution still uncertain, and of which the principal species have been described by M. Dumas and his co-labourers. I wish to speak of oxamethane and of analogous compounds, which furnish the oxalic ethers of divers alcohols.

These compounds, which contain in their equivalent the elements of an equivalent of oxamic acid, and of an equivalent of ether, may be conceived as ordinary compound ethers, into the constitution of which the oxamic acid enters, which forms the object of these researches.

In order to justify by facts this supposition, it is necessary to observe two things—either to obtain oxamethane and the analogous composition by etherification, made to operate directly by means of the free oxamic acid, or to withdraw the oxamid acid itself from the

decomposition of these ethereal compounds. I have not succeeded at present in obtaining oxamethane by the action of oxamic acid on alcohol, though some observations, which are yet imperfect, make me regard this transformation as possible; but I have the power, in retaliation, easily to extract oxamic acid from oxamethane and from analogous compounds.

In their researches on oxamethane, M. Dumas and Polydore Boulay have proved, that by ebullition with water this compound becomes transformed into alcohol and bi-oxalate of ammonia. But that which I have learned on the easy decomposition of oxamic acid, by the intervention of water at 100° , permits me to suppose that this bi-oxalate proceeds from a decomposition, secondarily undergone by the oxamic acid itself, produced direct from this reaction.

This supposition is, in fact, justified by experience. If we submit to ebullition an aqueous solution of oxamethane, by taking care to add, drop by drop, to the boiling liquor, a portion of diluted ammonia, in such a manner as to neutralize by this base the acid which is developed by ebullition, the liquors, thus maintained in a state of neutrality almost complete, will no longer contain anything when the oxamethane has been decomposed, except the oxamate of ammonia. Precipitated by the azotate of silver, it produces a gelatinous magma, which, by its appearance, its solubility in cold water, and the aspect of the crystals which it leaves deposited on cooling down, presents such an identity of properties with the oxamate of silver, that it is impossible not to conclude thence an identity of nature which justifies, for the remainder, the analysis of this salt.

0.470 of this salt, submitted to the action of heat, have left for a residue 0.257 metallic silver. This same quantity of oxamate of silver, obtained directly, has furnished 0.258.

0.585 of salt of silver, proceeding from oxamethane, have furnished carbonic acid, 0.270; water, 0.070.

0.507 of the same product, have furnished 32 cubic centimetres of azote, temperature 17° , pressure 757,8.

These results give, for the analysis of the salt of silver, numbers approaching very near to those which were furnished by the analysis of the oxamate of silver obtained in a direct manner.

The oxamethylene and oxamylene, composed from amylique alcohol, correspond to oxamethane, and which I shall make known in a short time; produce the same effects, and give place to the production of the same acid, in the decomposition which they undergo by ebullition with water.

This mode of decomposition of oxamethane has been alluded to by M. M. Dumas and Polydore Boulay. In their work, so remarkable on the compound ethers, they prove that *baryta, by acting on the oxamethane, disengages ammoniac, and forms a salt slightly soluble, but susceptible at all times of crystallization by the concentration of the liquor.* There is no doubt that this salt, the study of which the series of works executed by those chemists did not permit

them to enter into, was not, in fact, the oxamate of baryte of which I have already spoken.

If the knowledge of the oxamic acid clears the history of oxamethane and the analogous compounds, we may be permitted to hope that it will throw some light on the constitution of some azotic acids, and perhaps also on the true nature of those compounds formed by gaseous ammonia and the anhydrous acids. I have already made some essays in that direction. I shall have the honour, at some future period, of submitting the results to the judgment of the Academy, if they appear to me to be worthy of being submitted to it.

On the Influence of Interposed Pairs in the Voltaic Pile. By DOCT. BUFF, Professor at Guisseu. (Ann. der Pharmacie, tome 33, premier cahier.)*

After having reviewed the works of Jäger and Faraday, as well as those which I have made myself on the subject, the author seeks to prove, by new experiments, the theory which I have given, in order to explain the production of the electricity of tension and of dynamic electricity of a pile. He states that, conformably to my explication, there is always a relation to be established between the conductivity of the pile itself, and that of a body interposed between the poles. He shews, that by introducing in the interior of the pile indifferent pairs, that is to say, pairs made of the same metal, of copper, for example, the electricity of tension with which they are charged can only proceed from the poles themselves, and which are distributed through all the pile, conformably to the principle which I have advanced. It is by making an electrometer communicate with the different plates of the inactive pairs, when the pile is insulated; or when the poles are united by an imperfect conductor, that the author shews the exactness of the conclusion.

Thus, by introducing three inactive pairs into the middle of a pile composed of a number of pairs, he found negative electricity in that of the inactive pairs which was on the side of the negative pole, and positive electricity in that which was on the side of the positive pole; and at the same time, both negative and positive in the central inactive couple, the one on the first of two plates of copper of this pair, the other on the second of these two plates.

By studying the dynamic effects of electricity by means of the galvanometer, the author again found new proofs in favour of the principles to which we have referred. The interposed inactive pairs diminish the force of the current, without regard to the place into which they are inserted in the interior of the pile. When they are in a sufficiently great number, proportional to that of the active

* Archives De L'Electricité, No. 1.

pairs, the nature, more or less exciting, of the liquid with which the latter are charged, does not sensibly modify the force of the current. On the contrary, the slightest change in the nature of the liquid with which the inactive parts are charged, in this regard, exercise a great influence. Thus, a drop of sulphuric acid, put into each of the chambers of the inactive pairs, singularly facilitates the circulation of the current. If we put in diluted nitric acid, but still strong enough slightly to attack the copper, we may make the current from a single couple pass across three, or even four, inactive couples, without undergoing any sensible diminution of its intensity.

The active pairs were only charged with water in the preceding experiments. If we charge them with acidulated water, the same inactive pairs, which scarcely affected the current in the first case, arrest it almost completely in the second. The author attributes this result to the fact, that the interposed pairs possessed a conductivity sufficiently great to transmit the small quantities of electricity, but not to give passage, without augmentation of tension, to more considerable quantities of the fluid.

The author remarks again, that the more the liquids act chemically on the metals which unite the inactive pairs, the more the transmission of electricity is facilitated. He observes, likewise, that the intercepting effect of an inactive pair is as much less in proportion to the number already interposed.

There is not, however, anything very new in this work of Mr. Buff; but all the experiments which he has recorded agree with the principles which Mr. Faraday and myself have stated, on the origin of the power of the voltaic pile.

A. DE LA RIVE.

ROYAL VICTORIA GALLERY, MANCHESTER.

CONVERSAZIONE, DEC. 9TH, 1841.

Lecture on the Evaporative Power of Boilers.

By C. W. WILLIAMS, Esq.

The recent occurrences in this town, which were attended with such fatal results, have given the subject which I propose for consideration this evening a more than ordinary interest, and justify me in drawing attention to the details which I shall now submit to you.

In opposing the opinions of so many able men, respecting the causes and circumstances attending steam-boiler explosions, I will not presume to offer any merely theoretical views (my objects being *practical* results). I will confine myself, therefore, strictly to the statement of *facts*, as they have come under my own eyes, and drawing such inferences from them as will, I trust, be considered both legitimate and well founded.

The question I propose to consider is, the causes of those injuries to the boilers of steam vessels which have reference to their

deterioration and injury, and those which lead directly or indirectly to *explosions*.

Among others of those two classes of causes, I find the following as enumerated by many writers and practical men,—namely, the over-heating of the plates of boilers, and a series of dangerous consequences which such over-heating produces, and the creating a steam pressure beyond the strength of the boiler to sustain.

Of the latter little need at present be said: if the pressure of steam be beyond the strength of the boiler-plates, rupture or explosion must ensue. The former alleged cause, however, of the *over-heating*, more immediately requires consideration at present. Now, this supposed over-heating, and burning of the plates of boilers, is so directly connected with the circumstances attending the transmission of heat through them, that in examining this latter we shall find the direct solution of the former.

The transmission of heat through metallic plates or bodies, called "*conduction*," is too well known to require comment. The *degree* to which this conductivity may be exercised without producing any injury, and the effect which its various modifications may have on metallic plates, has not been sufficiently attended to, as far as *practice* is concerned, and to this, therefore, I will now apply myself, under the conviction of its paramount importance.

Experience leads me to conclude, on this head, that so long as the heat is transmitted *through* the metal in the same *quantity* and with the same *rapidity* that it is received, the metal may be considered as a mere carrier, and in the sense to which we apply the term when we speak of the passage of water through a tube or porous vessel.

This, I venture to assert, may be taken as a fact, without any regard to the *quantity* of heat so transmitted; at least, as regards all ordinary furnaces, and where an artificial blast has not been used to create an over-intensity of temperature.

If, however, the heat be by any means obstructed in its passage *through* the plate, or delayed in its exit *from it*, an *accumulation* instantly occurs, and the state, character, temperature, density, hardness, and strength of the metal are affected. The consequences and extent of injury will then be modified by other numerous and accidental circumstances.

The distinct and primary point for consideration, then, appears to be, what are the causes which produce this accumulation of heat in the plate; or, rather, what is it that causes its *obstruction* or *delay* in the course of transmission.

This view of the subject leads us to draw a distinct line, as to effect, between the body transmitting the heat and the body to which it is transmitted: in other words, between the facility with which the *metal* is endowed for *transmitting* the heat, and that of the *recipient* body for *receiving* it; and this also involves the *rate* at which the heat is absorbed or taken up by the recipient. (Mr.

Williams here illustrated, by an experiment, the truth of this proposition.)

This is an important distinction, as we shall find that the capacity of the receiving body is that which, in fact, governs the whole. In the case of boilers, water is so excellent a *recipient* of heat, though not considered a good *conductor*, that the rate of transmission will not be impeded; that is, the rapidity with which heat is conducted through a metallic plate (and which rapidity is essential to its preservation) will not be impaired by any delay or obstruction, on the part of the water, in taking it up. With such a recipient, then, as water, no injury can be sustained, either by the surface plates or other conductors, though exposed to the most intense heat of a furnace. Since injury to boilers, therefore, does not arise either from deficiency in the transmitting power of the metal, or receiving power of the water, we must look for its cause from some other source.

The most intense heat which can, practically, be found in a furnace, is that produced by a highly charged coke or anthracite fire, when in full action. If water remain in contact with a metallic plate, though the latter be exposed to the full action of this elevated temperature, the heat will be absorbed as rapidly as it can be transmitted, and the plate will remain absolutely unaffected and uninjured; but if any body, whose *receiving* power shall be inferior to that of water, be interposed between it and the plate, or if any accidental obstruction to the passage of the heat through the metal should intervene, a delay in the transmission will be induced—accumulation of heat in the metal will immediately take place—the plate will become overheated, softened, and ductile—expansion will follow—and hence, if there be any internal pressure from steam, warping, bulging, or rupture, will ensue.

Let us now examine the effects of interruption in the transmission of heat, by reason of a defect in the *receiving* capacity of the body to which the heat is transmitted or conducted. The leading one, to which I am about to direct attention, is that *incrustation* or *deposit* which settles in the interior of boilers, and deranges the transmission of heat. I will, however, say a word on the supposed effect of an over-heated furnace, and of allowing the water to fall below its proper level.

As to the first, it is quite groundless. No damage can be done to the boiler by reason of excessive heat, or from any other cause connected with the fuel or the furnace, beyond the ordinary wear and tear, *provided due attention be paid to the mere circumstances of preserving the interior in a proper state of cleanliness*, (of which I will say more hereafter,) *and maintaining the water at its proper elevation*. I have made every effort to injure the bottom and flues of boilers, by all degrees of temperature in the furnace, and all sorts of changes in the situation and management of the fuel, but without producing the slightest effect, when these two conditions are properly attended to. I have an experimental furnace and boiler at

work in this town, and I defy any individual, by any management or mismanagement of the furnace or fuel, to injure the bottom of the boiler, provided they look after the water in the interior. Only let me have the management of the interior, and I will allow free access to the exterior.

With respect to the second source of injury, "allowing the water to fall below its proper level," nothing need be said on the subject; its consequences are too manifest to require comment. The other source of injury—*incrustation*—demands, however, a closer investigation. The reasoning on this head most prevalent is, that the incrustated deposit which adheres to the plates, in the form of a solid hard crystallized body, is a bad conductor of heat; and that owing to this circumstance, the plate on which it is deposited, if exposed to the strong action of the fire, as in the bottom of cylindrical boilers, has a tendency to become over-heated or burned. It is by many supposed that this incrustation, being suddenly separated or driven off, and occasionally in large quantities, the water is thus as suddenly brought into contact with the supposed over-heated plates; that a sudden and commensurate generation of steam is the consequence, and that expansion, bulging, rupture, or even explosion, may ensue. To these allegations, and the inferences drawn from them, I demur, as inconsistent with the real state of things, and irreconcilable with the laws which govern the transmission of heat.

That incrustation, which is usually a carbonate or sulphate of lime, adheres to the bottom, sides, and even roofs of the boilers, is an established fact; but that *any of the alleged results follow*, appears to me to be unsustainable. It is assumed (but certainly has not been proved), that the plate beneath the incrustation, and more especially in the parts most exposed to the radiant, and necessarily greatest heat of the furnace, becomes *over-heated*, and even *red-hot*, by reason of its non-conducting character. How this non-conducting fact has been ascertained, remains to be shewn. I have met those who assert that they have actually seen the plate *red-hot*, notwithstanding there was a sufficiency of water in the boiler. That they have seen a red-hot plate, I will not doubt; but it is the inference which I deny—namely, that this over-heating has been caused by the *incrustation*, or any circumstance connected with the furnace.

My first cause of doubt as to this *assumed*, and, as we all thought, undisputed fact, arose from the circumstance of perceiving that several of our boilers, which were always, and to the very last, good generators of steam, were overloaded interiorly with a mass of incrustation, but in which boilers no injury of any kind had ever occurred, neither bulge, nor crack, nor rupture, being perceptible, even when the boiler was fairly worn out by age.

I began, therefore, to doubt this theory of over-heating, or that incrustation could possibly be its cause, and I determined to test the correctness of the hypothesis by experiment. For this purpose, I

constructed several vessels (some of which I now exhibit), having the bottoms formed of pieces of this very incrustation, from $\frac{1}{8}$ to $\frac{3}{8}$ in thickness. I also constructed similar vessels with iron-plate bottoms, and of corresponding thickness. These vessels, being filled with water, were successively exposed to a very great heat, and which was confined exclusively to the bottoms.

The result was, that the theory of over-heating by reason of the incrustation was at once found to be erroneous, the incrustation, instead of a non-conductor, being found to be substantially a good one.

In comparing the conductivity of *iron* and *incrustation* bottoms, I found the difference so insignificant, that I could no longer believe that injury to the metal could possibly result from the latter. I also found that, even when the water was at the full boiling, the heat of the incrustated bottom was not sufficient to injure the hand when touched.—[Mr. Williams here took the vessel from the flame over which the water was boiling, and laid his finger to the bottom formed of incrustation. Several other gentlemen did the same, and pronounced it to be at too low a temperature to be injurious.]

To what, then, is attributable the well-known fact of over-heated, and even red-hot plates, and their consequent injury? It is here worthy of remark, that in *marine* boilers *bulging* or *rupture* is never known except when there is a deficiency of water; or, which is equally injurious, when, from the peculiar construction of the flues, the free circulation of the water is impeded.—[Mr. Williams exhibited a model of the boiler of the steam vessel the "*Athlone*," to show the liability of marine boilers to the injury arising from the want of free circulation of the water, and from which cause so much over-heating and bulging of the plates occur.]

This over-heating, then, in land boilers, may with certainty be attributable to that other species of deposit which does *not* assume the form or character of a crystallized body. This, then, is the body which, under certain circumstances, and from its non-conducting character, forms the main obstruction to the transmission of the heat, and its consequent accumulation in the iron until over-heating and redness ensue.

It will be observed, that the deposition from the water, which is invariably some of the salts of lime, may thus be divided into the crystallized and the uncrystallized portions. I have shewn that in the former it is a good conductor; in the latter, however, it is the reverse. Whether this arises from the porous nature of the body itself, or merely from its *accumulated mass*, is not so easily ascertained. In this uncrystallized state it forms a loose, unconsolidated, earthy, friable compound, being a muriate, carbonate, or sulphate of lime, and merely held in *suspension*.

The danger, however, commences when this matter (though perfectly innoxious while in this suspended state, and so long as the water is in motion by the act of boiling) is allowed to subside, by

the boiler being at rest. It then becomes a compact and indurated mass, resembling the anhydrous sulphate of lime (commonly called *Plaster of Paris*), after having been supplied with a due portion of water; in fact, after what is called "setting" has taken place. Subsiding while in the state of powder, it settles and consolidates at the bottom of cylindrical boilers, and the legs or sides of waggon boilers.

In this state of rest, and when, by the internal heat of the boiler, it has dried and consolidated, it becomes almost impervious to, or at least a very slow absorbent of, water.

Mr. Williams here exhibited a cylindrical and solid body of this apparent concrete, which had been in a state of suspension the preceding day in boiling water, but which, on being allowed to cool, and become thoroughly dried, presented a hard compact body, of the exact shape of the vessel in which it had subsided, and so hard as scarcely to be marked by the nail.

In this state it becomes a mischievous non-conductor, and not unfrequently being found many inches thick, it effectually prevents the necessary access of the water to the plate, when the boiler is again put into action; thus producing the whole chain of events, from an over-heating of the plate to its bulging or rupture.

For the purpose of establishing this fact, I made some vessels, the iron bottoms of which were so inserted, by means of solder, that on the event of the iron becoming over-heated, the solder would melt, and the bottom fall out. The result was as anticipated, and a deposit of but a single inch in thickness, after being allowed to settle and become hard and dry, was found sufficient to cause such an over-heating of the plate as to melt the solder.

Under such a state of things, if the plate so exposed to over-heating be a *sound* one, it will bulge with an uniform and regular curve, by force of the internal pressure of the steam; the exact shape or figure of the bulging being the result of accidental circumstances. In this state no injury will be sustained; the plate will remain as strong as before, and as if such bulged shape had been given to it intentionally in the first instance.

Mr. Williams here exhibited a very large iron plate, eight feet in length, illustrative of the bulging, but which had not in any way weakened it.

If, however, the plate be an *imperfect* one, that is, presenting in its section a laminated and unwelded structure, the bulge will tend more or less towards a crack or rupture, and, becoming necessarily weaker, the two portions, or laminæ, separate; an additional obstruction to the passage of the heat is thus created, and the plate will give way at the weakest spot.

This will be found to be the case with the majority of injured, cracked, or ruptured plates, and which are very erroneously and ignorantly then said "*to be burned.*"

In exhibiting to Mr. Parkes the fact of the good conducting quality of the incrustation, he pointed out an oversight in the experiment; namely, the necessity of having both iron and crystallization in contact, and taking their *joint* conducting powers as they exist in practice. To meet this case, I had a vessel made with a bottom formed of incrustation, five-eighths thick, to which I added an iron bottom of the same thickness, the bottom being thus $1\frac{1}{4}$ inch thick; the result, however, was as before. This great thickness presented no impediment to the passage of the heat, and when the water was at full boiling temperature, this joint iron and incrustation bottom, though exposed to a very strong heat, was so little heated, that on being removed from the flame, the finger was pressed against it without injury. [This was done by Mr. Williams before his auditory.]

In a paper by Mr. Parkes, (see the *Inventors' Advocate* for July 31st, 1841,) in reference to the late boiler explosion at Gateshead, he observes, "It appears from the evidence given at the inquest, that the internal state of the boiler, as manifested by the *quantity of deposit ejected*, is quite sufficient to account for its dislocation."

Now, had this matter assumed the form of an *incrustation*, no danger or over-heating could have ensued. But he further goes on to say, "I have referred to a boiler incrustated, but a similar sudden disengagement of steam, in large volumes and of high elasticity, will result in the *disturbance of a covering of mud*."

Now, this is quite to the point, for here we have a positive non-conducting body interposed between the water and the plate, the necessary consequence of which must be, overheating and all its train of injuries.

Under such circumstances of deposit, we find that the accidental interposition of any solid body on the boiler has the direct effect of aiding the collection of this deposit around it, and consequently producing a local accumulation of this non-conducting medium. I myself witnessed a rupture of a plate occasioned by the neglect of a workman in leaving a large sledge hammer in the boiler. An accumulation of indurated deposit was found collected at the spot, enveloping the hammer head, and causing the bulging of the plate immediately under the spot.

Now, what inference are we to draw from these facts? and how can we apply them practically to remedy or avoid the danger from such deposits? Either by preventing this dangerous subsidence, by collecting it *while in suspension* in the boiler, or by a proper attention to its *removal* before it has been allowed to become dangerous from its quantity and solidity.

Many years back, Mr. Williams observed, his attention had been drawn to the means of collecting this floating matter while the boiler was in action, by the introduction of vessels into the boiler, and which he had tried with the most decided success. In marine

boilers, it was, however, useless and even dangerous, unless the operation could be performed mechanically, and without the attention of human agents.

Mr. Williams illustrated his observations by placing over the flame of a powerful Argand burner one of the vessels, the iron bottom of which was soldered in, and in which a portion of this indurated deposit had been allowed to settle. The result was, that this deposit being a bad conductor, the iron became over-heated, the solder melted, and the water ran through.

When this matter, therefore, (Mr. Williams assumed,) has taken the crystallized state, it may be said to have lost its power of producing mischief to the plate—the reverse, however, when un-crystallized.

But incrustation never adheres, to any dangerous extent or thickness, on those parts which are so exposed to a high temperature that they could be over-heated, or approach to redness. This is an important fact, and opens a new view of the subject, as regards the formation of the deposit. My inquiries lead me to this conclusion: that accumulation, in the crystallized state, takes place by *two distinct processes*, according to the temperature and character of the plate, and the part of the boiler where such crystallization is formed, and after the following manner:—When the heat is moderate, and the iron not subject to much fluctuation of temperature, or the boiler being set at rest, or the fire being again lighted, as, for instance, on the roofs or parts furthest from the fire, the successive layers or strata are formed *over* each other, and, for the most part, bearing the character of one uniform density. When, however, the heat is the greatest, as in those parts immediately over the fire, or adjacent to it, and when the iron is subject to greater extremes of expansion and contraction, the incrustation remains attached to the plate but a short time, and arrives at a very moderate thickness. This alternate contraction and expansion so deranges the adhesion, that more or less of moisture or water insinuates itself between the incrustation and the iron plate; and the next time the boiler is set to work, the incrustation is instantly dislodged, and the successive layers or strata are then formed *under* the preceding ones. Thus nature comes to our aid, and by this process of disunion or disconnection, the plates are freed from the incumbrance, and the steam is generated *under* such previous layer of incrustation, until, finally, the accumulated mass breaks off and falls to the bottom.

Mr. Williams presented several specimens of these accumulated strata, illustrative of this under formation, the inspection of which manifestly disproved the idea of the additional crystallization having been formed *over* or *on* the preceding ones.

GALVANOPLASTIK ; or the Process of Cohering Copper into Plates, or other given forms, by means of Galvanic Action on Copper Solutions. By Dr. M. H. JACOBI, Privy Councillor to the Emperor of Russia, and Member of the Royal Academy of Sciences of St. Petersburg.*

(Continued from page 498 of vol. vii.)

VII.

We see, from the management, that, however simple the process of obtaining galvanic copper plates may appear, it requires very much attention. We shall also find, by closer inspection, and especially by practical exercise in the art, that many interruptions take place, which it becomes desirable to remove; for, in the first place, as already stated, there is a limit with reference to the saturation of the cupreous solution, the strength of the current, and even in the time in which we can obtain the deposited copper plates. The great tenacity and firmness which the copper attains by slow reduction, is certainly a desirable property, though only of a subordinate consideration where it operates as an impediment to the acceleration of the arts.

If, by some oversight, the precipitated copper should become of a brown red, or be loose, a new layer, even from a saturated solution, will not attain the required hardness and colour. In such cases, the brown surface must be scraped off, or the whole plate be well washed over with diluted nitric acid. The most serious mischief, however, by the hitherto described method, is, that it is too much limited in its operations, both as respects configuration and magnitude of the plates produced; and, in order to obtain a constant and economical action, the zinc, and the original article to be copied, must have placed between them one of the described forms of diaphragms, which ought to keep the fluids separate, and yet keep its pores open so as to permit free circulation. When the impediments in this part of the apparatus are but trifling, they are easily removed; but when considerable, they are attended with many difficulties and inconveniences, and not easily got rid of. According to this first method, it is probable that the galvanoplastik art might have had but a very limited field of operation: at this stage, however, was the beginning of my troubles. Fortunately our success does not depend upon first efforts, nor ought we to consider that the impediments which first present themselves will eventually be of much practical consequence.

In order to comprehend upon what principle the second method depends, it will be necessary that I say a few words on the chemical action of the galvanic currents. It has already been stated, that

* Translated from the German edition.

within the closed circuit, and, it is true, in the cells where the excited plates are placed, there is a state of chemical activity. This chemical activity is not, however, limited to this particular part of the circuit, but allows of being transferred to any other, wherever we place an intermediate cell, filled with a conducting liquid, through which, by means of homogeneous metallic plates, the galvanic current can be transmitted. It appears from this, that nothing more is necessary to shew this fact, than that the metallic conductor, which goes from one pole of the battery to the other, should be somewhere interrupted by a liquid conductor. If we allow that the metallic plates, which are immersed in this liquid, are perfectly homogeneous, so that they cannot possibly excite a galvanic current, the following law invariably takes place:—*The hydrogen of the liquid, or the element which represents it, is developed at that plate which is connected with the zinc pole; and the oxygen, or its representative, goes to the other metallic plate, or that connected with the copper pole of the battery.*

The celebrated English philosopher, Faraday, has given to those plates, which are placed in the liquid to be decomposed, the denomination of *electrodes*; and, indeed, he names that plate which is connected with the zinc, the *cathode*, and the other, which is connected with the copper, the *anode*. In consequence of these abbreviations, we shall employ them in the remaining part of this work. Fig. 25: A B represents a galvanic battery; C D is a vessel nearly filled with diluted sulphuric acid, in which are immersed the two platinum electrodes, *a* and *c*. Of these, *a* is the anode, and *c* the cathode. If the current be sufficiently powerful, *c* will liberate hydrogen, and *a* will liberate oxygen. If instead of a solution of sulphuric acid, we place a solution of sulphate of copper in the decomposing cell C D, the laws of action will be such, that the plate *a* will, it is true, still liberate oxygen; but *c* will now become covered with a film of metallic copper; whilst the hydrogen, as has before been stated, will return to be employed in the reduction of the copper. If instead of the platinum electrodes, we place two copper ones, with diluted sulphuric acid, in the cell C D, the anode will develop either none, or but very little, oxygen; because, under these circumstances, it has very great affinity for the copper, which it oxydizes, and then becomes dissolved in the sulphuric acid. The cathode will, at first, liberate hydrogen gas as usual, but the development of this element will become gradually feebler, and shortly it will disappear, and in its stead the surface of the cathode will become covered with reduced copper in a loose brown-red condition. The fluid itself, which at first was clear as water, becomes gradually of the blue colour of the sulphate of copper. Although I carried on this experiment for several successive days, a slight development of gas at the cathode was still discernable; which demonstrated that, by this apparatus, the reduction of the copper goes on only in an incom-

plete manner ; even the precipitate does not become coherent, but granulous, which is probably due to the bubbles of hydrogen preventing the intimate connection of the cupreous particles. It is otherwise, however, when instead of diluted sulphuric acid, we employ an almost saturated solution of the sulphate of copper. In this case, however, the anode also dissolves, whilst the cathode bedecks itself with completely coherent clear red copper, as complete even as by proceeding according to the first method. The copper solution, by this means, continues in a state of saturation always proportional to the progress of reduction, by a supply of copper from the dissolving anode. A comparison of the weights of the plates shows, that the cathode obtains nearly the same quantity of copper as that which the anode loses. The small, perceptible difference is too insignificant ever to become a consideration in a practical point of view.

Since by this arrangement of apparatus we are enabled to form copper plates in a vessel entirely separate from the battery, and entirely dispense with the large porous diaphragm used by the first described method, we arrive at an almost unlimited freedom in reference to the size and shape of the original from which we wish to take copies. Nor is the advantage less in reference to the time necessary for the operation, which by this means is much shortened ; so that daily a film can be deposited of nearly a line or $\frac{1}{80}$ of an inch thick, and in many cases it will reach the thickness of a common card. To accelerate the reduction still more than this rate, appears not to be prudent, because of its affecting the firmness of the copper. When the current is sufficiently powerful to reduce two solotnik of one square inch in twenty-four hours, we obtain a loose incoherent product. It is possible, however, that by the employment of other salts of copper, we might obtain very different results.

It is a fortunate circumstance, that for the decomposition of the copper solution between the copper electrodes, a battery of one pair only need be used. We can, it is true, employ more pairs in series for the purpose of accelerating the action ; in the meantime, it is to be understood these laws will take place, viz.,—in proportion as the pairs of plates in series are augmented, more zinc becomes consumed: not only in one cell, but in every cell in the series does this augmented action take place.

The copper solution which is placed between the copper electrodes must, to a certain degree, be diluted, which is best ascertained by experience. If, at the commencement, we employ a completely saturated solution, we shall observe, sooner or later, that the needle of the galvanometer indicates a tendency to return to the meridian, or at least it begins to be very unsteady. When this is the case, and the unsteadiness of the needle is not occasioned by any irregularity in the battery, we must dilute the solution with water until the deflection of the needle becomes more steady. It is

sometimes useful to add to it a little sulphuric or nitric acid. The anode or the copper-plate that dissolves, generally becomes covered with a blackish or dark brown powder, which, if it falls down on the cathode, occasions the reduced copper to be brittle. This circumstance is easily avoided by placing between them a partition of flannel, linen cloth, &c. &c.

Notwithstanding the arrangement of this decomposing apparatus, no great difficulty is presented, since it can be disposed in many different ways; and as the most usual way is to place, in connection with the battery, the two copper plates, of any chosen form, opposite one another, in the decomposing cell, nearly filled with a solution of sulphate of copper, I shall, in fig. 26, shew the arrangement of the apparatus with those plates, arranged in horizontal positions. In this figure, *a, b, c, d*, represents a vessel or reservoir holding the is not seen in the figure. This vessel may be of glass, porcelain, or even of wood, if made water-tight by a lining of asphaltum or resinous cement. We may also use lead or copper for this purpose, taking care to line such vessels with plates of glass or asphaltum cement; *e, f*, represents the cathode, which serves as an original to be copied, and whose surface is intended to be covered with the reduced copper. This plate, by the conductor *f, g, h*, is connected with the zinc pole of the battery, which, as already stated, may consist of a pair of plates, of small or large dimensions, according to circumstances; *i, k*, represents a flannel or linen partition, stretched on a wooden frame, on which the copper plate, *l, m*, is placed: this latter plate is connected with the copper side of the battery by the conducting wire, *n, o*. Instead of a whole plate, we may employ even single fragments, which ought to be so placed as to touch one another. In the circuit is placed the magnetic compass, *y*. Let me remark, in this place, that the process is not so satisfactory when the distance between the plates is less than about an inch and a half or two inches. When the dimensions of the plates are pretty considerable, for instance, a foot and a half or two feet square, the distance between them may be allowed to be three or four inches, which would not be too much.

The use of the compass is not sufficiently valuable to recommend in all cases; it will become directly useful, probably indispensable, when the galvanoplastic art has become a profitable source of employment. When this period arrives, it will be necessary, at least advisable, to prepare a number of these simple instruments at the commencement of any extensive process. If we combine several decomposing apparatus, of equal or nearly equal dimensions, and the same number of single batteries in series, in the galvanic circle, we have no need of more than one compass; as may be understood by inspection of fig. 27. By this arrangement, nearly the same weight of reduced copper will be found on each plate, at one and the same time. This circumstance also takes place even when the plates are of unequal dimensions; so that the thickness of the films obtained

are inversely as the surfaces of the plates. From the laws exhibited in a previous part of this work, it is obvious that the smaller the decomposing apparatus, the greater the resistance to transmission. We also enfeeble the current, and check the reduction of the copper, by combining together a great number of plates in the decomposing cells. Nor is it advisable to combine many decomposing apparatus together with one battery, as represented by fig. 28, because the distribution of the action is not proportionate to the whole force employed; and they are also liable to many accidental circumstances not easily avoided.

By this second method, we must employ galvanic batteries that will afford a constant action, such as those described in section 6; or at least formed upon the same principles, having a diaphragm for the separation of the liquids with which it is charged. The manifold and various kinds of arrangements that I have hit upon, whilst pursuing other applications of galvanism to a certain extent, I shall describe on another occasion; nevertheless, I shall give a representation of one battery in this place. In fig. 29, *a, b, c, d*, represents a rectangular copper or leaden box; *e, f, g, h*, is a cylinder, also of lead or thin sheet copper, which touches three of the sides of the box. It is better to leave the cylinder open at the side, than to solder or rivet the edges of the sheet together, in order that it may be easily opened and bent, so as to remove the reduced copper with which it soon becomes covered; *i, k*, represents a cylinder of potter's clay, and *l, m*, a cylinder of zinc, which may be either solid or hollow; *n, o*, is a small box of either lead or copper, perforated at the sides and bottom like a sieve. It is for the purpose of holding bruised crystals of sulphate of copper. When the liquid is wanted to be let off from the apparatus, we can use a flexible tube such as has already been described. Usually, however, I employ another contrivance for this purpose, which operates upon the principle of the syphon, and which I will here explain. It may be employed for many other purposes, and, I believe, in this form it is quite new. In fig. 30, *a, b, c, d*, is a vessel, with its draining tube *e, f*, which passes through the bottom of the vessel, and whose upper orifice is on the same level as the surface of the liquid; *g, h*, is a tube, closed at the upper and open at the lower end. This I call the *inverted tube*, (*Stülprohr*).^{*} The inner tube will discharge the most diluted part of the cupreous solution, by its overflowing at the top, when the outer tube is not in its place. If we use an apparatus to which no perforated magazine of crystals is attached, it will be necessary to employ a funnel with a long neck, when a new supply of the saturated solution of sulphate of copper is wanted, for the purpose of placing it at the lowest part of the apparatus, and thus lifting the old liquid so as to run off. If the whole of the liquid is to be discharged, the *inverted tube*, after being filled

^{*} This word might be translated *capped tube*, but I have chosen *inverted tube*, as more likely to be understood.—TRANS.

with liquor, is to be placed over the inner one, as seen in the figure, and thus the syphon is formed, through which the whole of the liquid which stands above the lower end of the inverted tube will become discharged.* It is scarcely worth mentioning, that the space between the top of the inner tube and the outer one must not be too small for the free flow of the liquid between them. When every part of the apparatus have the proper proportions, the discharge of the liquid need not depend upon its surface being above the level of the upper surface of the inner tube, because, by a motion up and down of the inverted tube *g, h*, the tube *e, f*, is easily filled, and the syphon thus brought into full play. This contrivance is very convenient for the purpose of charging several vessels at the same time: the inverted tubes are all arranged and fastened in a frame, in order that they may be let down at once. The discharging tube, *e, f*, must not be too wide, so that it may easily get filled. The solution of zinc will gradually become more and more saturated, if care be not taken to remove it and add water in its place, which must always be attended to. It may very easily be diluted, by pouring fresh water to the bottom of the vessel through a long-necked funnel, by which means the newly-added water gets beneath the saturated solution and lifts it up, mixing, at the same time, to a certain extent, with the lower strata. If it be thought proper to remove the whole of the zinc solution, the porous vessel must be taken out, and its contents poured off, which is but seldom necessary, as may easily be shewn by fig. 31, in which is seen a syphon, similar to the former, for removing the solution of zinc when required. The bottom of the copper vessel, *a, b, c, d*, is furnished with a small attached tube, in which another tube, passing through the bottom of the porous cylinder, *e, f, g, h*, is placed and cemented. Into the upper orifice of the latter is cemented a glass tube, *i, k, l, m*, whose upper end reaches to the level of the surface of the liquid. Both this and the outer tube, *n, o, p, q*, are best made of glass, and fastened, by some means, so as to prevent derangement by agitation; also constantly left open, so as to be ready, on all occasions, to let the old liquid run off in the direction of the arrows, when fresh supplies of saturated solution are introduced. Should it be desirable to remove the whole of the liquid, the last-named tube may be taken off, and replaced by another, similar to the inverted one *g, h*, in fig. 30, by which means the discharge will be as complete as by the former method. If there be sufficient room in the porous cylinder, *e, f, g, h*, the inverted tube may be sufficiently large to be placed over the outer glass tube, without removing it from its place.† At an early period I employed an arrangement which I shall describe at another convenience; I may here state,

* This contrivance, although new to our author, is very old in this country. The syphon in *Tantalus's Cup* is very often made in the same manner.

† We are of opinion that our author has made his apparatus rather too complex to become of general use.—TRANS.

nevertheless, that the one now described stands next for convenience and simplicity. There remains to be made known, however, that by this arrangement there is one disadvantage experienced, by our not being able to renew the porous vessel without interrupting the action of the whole apparatus: and it is necessary, now and then, to change these porous vessels, and soak those last used in clean water, in order that their pores may be well cleansed of the crystallised salts that lodge in them. This process will occupy five or six days, according to circumstances; which, however, I have found is a sufficient length of time, even after I have kept a battery frequently from fourteen days to three weeks in uninterrupted activity, and in such uniform action, that the small deviations in the direction of the magnetic needle, and variations of temperature (*Temperaturveränderingen*) in the liquids, were too trifling to describe.

The size of the apparatus here spoken of becomes limited, in consequence of the difficulty in procuring porous cylinders of considerable dimensions, and at the same time having thin sides or walls; thick walls offer a considerable resistance to the currents, and consequently lessen the action. It is, therefore, preferable to unite several smaller apparatus into one common action, by connecting all the copper cylinders together on one hand, and all the zinc cylinders on the other, so that they may form but one voltaic pair. Fig. 32 represents a ground plan of an apparatus of nine elements, placed in one common reservoir, and which can be properly united by clamp screws, or by any other convenient method, so that they may form *one single pair*, of nine times the surface presented by each individual element. The method of combining the metals is not shewn in the figure, in order to prevent confusion. It is true, this apparatus requires much liquid to keep it in action; notwithstanding, however, it is so much the more durable, and is easily managed.*

For whatever purpose the galvanoplastik art may be used, we shall see, by what follows, that it is capable of very extensive application. The hitherto known methods of obtaining metallic copies, by casting or stamping, may be entirely superseded by this process; and the certainty, sharpness, and accuracy, in every case, is such as by the old methods could not be accomplished; and in regard to the beauty of polish, it surpasses coins or medallions from the finest steel dies. If we take a plate of gold, silver, or copper, of the highest polish, and employ it as a cathode in the galvanic circuit, the copper that becomes reduced upon it, not only exhibits the exact polish of the original, but it finds out the finest microscopic scratches which the polishing process left unfilled up. If we touch with the finger a highly polished plate of metal, a small dull speck,

* There are a few more lines of this section, which it would be useless to translate, as they merely refer to the supposed cost of keeping up the action of the apparatus in Russia, by turning to account the sulphate of zinc and other products. —TRANS.

which shews the pores of the skin, will be left behind : even this speck will be so accurately copied by the galvanic process, that the keenest microscopic observation can detect no difference from the original. It is well known, that to obtain metallic copies by striking with dies, is a very expensive process, and the truth of the original is always encroached upon ; but by the galvanic casting the most accurate copies are produced at a very trifling cost.

When we are about to multiply copies from a deeply engraved plate, we must, in the first place, make a galvanoplastik picture, *in relief*, of the original engraving ; and upon this first galvanoplastik we can again, at pleasure, obtain other plates, which will be precisely identical with the original ; and they may be printed from it in the same manner, and the impressions will be as good as those taken from the first engraved plate. I will also make a few remarks in this place respecting the separating of the galvanoplastik from the original plate. The two will always separate very well, when the new plate is allowed to become sufficiently thick, if the copper of the original be not spongy, porous, or scaly, and, finally, if the design be not engraved too perpendicular, or, rather, be not undercut. There are essential circumstances to be attended to in all cases where engraved copper-plates are intended as models for forming galvanic copies on.

If the galvanic copper be slowly and cautiously reduced, it attaches itself very excellently to every part of the engraved plate, and allows of being easily loosened. There is no difficulty whatever in making galvanic plates, especially where an already engraved copper plate serves for the original. By this means we get rid of the tiresome labour of grinding and polishing, requisite processes which the original engraved plate has already passed through ; at the same time, such original plates should have obtained an uniformity of texture which surpasses that possessed by the best mercantile copper. With common copper, it frequently happens that the galvanic plate adheres so firmly to the original, if not throughout the whole surface of contact, at least in many places, that the separation of them can be accomplished by no other than forcible means, and it is possible only at the expense of the original. Sometimes a thin film of fat, or oil, laid over the original plate, prior to its being submitted to the galvanoplastik process, facilitates the separation of the two. Strong coverings of wax, lac, &c., can only be used where sharpness and accuracy is not of much consequence. The galvanic plates are easily removed from silvered or gilt plates, on which they have been formed. It appears, also, that for this purpose the gilding performed by the wet process has an advantage over that in which the amalgams of gold are used. An exposed covering of thin gold leaf, or gold size, over the face of the copper, answers, also, to facilitate the separation, and may be used for that purpose, where it can be done without injuring the sharpness of the design. These precautionary measures, after all, are not necessary

when the copper of the original is of a good quality. As soon as the galvanic plate has acquired the requisite thickness, and the parts grown over the edges of the original are filed away, it springs loose of itself, without any further trouble. It hardly need be mentioned, that a casting of gypsum would answer all the purposes of copper plate, provided it were nicely managed, and taken from an original engraving that was not under-cut.

(To be continued.)

On Voltaico-Electric Quantity and Intensity. By Dr. BUFF. (Ann. der Pharm., t. 32, p. 1.*)

The author has had in view the study of that species of mysterious property which Faraday conceives to be found in voltaic electricity, by virtue of which it is more easily transmitted; a property which he has named *intensity*, and which he distinguishes from *quantity*. He has found that this property is, as I have already indicated, only an effect of conductivity, by virtue of which the neutralization of two electricities accumulated at the poles of the pile is more or less easy.

The experiments of Dr. Buff are not all new, but they are interesting, as they confirm those which have already been made on the same subject by other philosophers.

He arrived at the conclusion, by varying the number of pairs in a pile, that if the circuit is closed by an imperfect conductor, the facility which this possesses of transmitting the quantity of electricity which can be developed by the battery is universal in proportion as the number of pairs is augmented.

The effects of tension observed by Dr. Buff are more numerous and more remarkable. He has at first studied them on a pile, the poles of which were insulated. The centre pair gave the same tension as it possessed when alone; two pairs, apart from the middle of the pile, gave a stronger tension; three, a still stronger; and so on to the poles, where the electricity attains its maximum. The author appreciated the electrical state of each pair in diverse manners, and in particular by means of a copper wire soldered to zinc, which he put in contact with the inferior plate of the condenser, whilst he touched the superior one with the finger.

By making the poles communicate with each other by distilled water, the same distribution of free electricity in the pile has been found, only it was a little more feeble. With acidulated water placed between the poles, and which was decomposed, the electricity of tension was still found in those pairs nearest to the poles. But no portion of it could be found when the poles had been metallically united.

*Archives De L'Electricité, No. 1.

The same effects have been obtained by charging the pile with diluted sulphuric acid, in place of ordinary water. By reuniting the poles by distilled water, the tension was still feebler, and no decomposition took place in any part; but when reunited by acidulated water, decomposition took place in all parts, in the troughs of the pile, as well as in those into which the poles were inserted. The tension has in all cases been stronger at the poles.

The author remarks the analogy which exists between these effects and those of an ordinary electric battery, charged with electricity from a machine; and he thence concludes that the greatest tension, or the *static intensity* of the pile, only proceeds from the resistances which the circulation of the electricity meets with. We conceive, then, that it is at its maximum at the poles, and that, without being obliged to attribute to the current a mysterious property, there exists for the two electricities, accumulated at the two poles, a tendency to neutralization, more or less strong, and consequently a greater facility for the current which results from this neutralization to traverse the imperfect conductors.

On an Advantageous Manner of Constructing Grove's Pile. By C. A. GRUIL. (Ann. der Physik, tome li. p. 381.)*

The author at first constructed this pile by placing a cylindric ring of platinum, in a porous earthen tube, closed at its lower extremity, and filled with nitric acid. The porous tube was introduced into a cylindrical jug, filled with diluted sulphuric acid, in which was also inserted a cylinder of amalgamated zinc, which enveloped the tube without touching it. This piece of apparatus, which is almost similar to a calorimeter, gave powerful results, but it presented this inconvenience: the thin layer of nitric acid, comprised between the surface of the cylindric ring of platinum and the interior partition of the porous tube, became rapidly heated, entered into a state of ebullition, and emitted nitrous vapours.

The author then substituted, for the cylindric ring of platinum, two plates of the same metal, cutting each other at right angles, following the axis of the tube, and forming a cross. He has, even at a later period, added a third plate, in such a manner that the three plates crossed each other at the axis, by making angles of 60° with each other. They were thus disposed around the axis, like the leaves of an open book round the back of the book.

After this disposition, all the nitric acid being in the circuit was much less heated. This pair would act during several hours without any disengagement of gas or decrease of the force of the current, when even the nitric acid had taken a deep green colour.

* Archives De L' Electricité, No. 1.

A Register of the Electric State of the Atmosphere, in relation to its Meteorological Phenomena, for the Month of June, 1841. By W. H. WEEKES, Esq.

JUNE, 1841.

Day of month.	Time in hours and minutes.	Thermometer.	Barometer in inches and hundredths.	Wind, its direction and character.	Weather and modifications of clouds.	Electrical condition of atmosphere.	General Remarks.
1		74	29.90	mere zephyr, W. by S.	Mountainous masses of cumulus cloud.	positive	An occasional low current, causing slight divergence until 2 P.M.
2		76	29.95	zephyrs, W. by S.	Blue sky, with numerous beautiful modifications of cirrus.	positive	Moderate divergence until noon.
3		74	29.30	zephyrs, N.W.	Blue sky, with here and there cirrose fibres.	positive	Divergence uniform but trivial.
4		66	29.90	moderate breeze, N. by W. S. by E.	Very fair, with cumulus cloud.	positive	Steady repulsion of gold leaves.
5	noon	66	29.90	breezes, N. by W.	Thin haze, — sun breaking through at times.	positive	Remarkably steady divergence until the approach of sunset.
	6, 15 p.m.		29.70	direct N.	Successive gentle rains.	negative	Steady divergence, moderate.
6		60	29.60	breeze N.	Cloudy, — rain drops at times.	positive	Pendulums in state of constant divergence, and frequently discharging.
7		58	29.50	breeze N.	Cumulo-stratus.	positive	Strong divergence, with an occasional intermission of a few minutes, when the gold leaves lose their repulsive condition <i>suddenly</i> .
	11 a.m.				Hasty showers of small rain.	negative	Sudden divergence on the approach of a ragged cumulus.
	11, 9 a.m.					positive	Streams of moderate sparks from terminus.
	1, 20 p.m.					positive	Increased energy of electric current for the space of an hour, followed by a laboured and heaving motion of the gold leaves.
	3 p.m.			moderate gale N. by W.	Hail shower. Frequent hasty showers.	variable	Torrent of brilliant sparks.
8		60	29.60	very little at times, N. by W.	Cloudy dark sky.	positive	Discharge of sparks as hitherto, during the showers, negative; with a strong positive current in the intervals.
9		62	29.50	light breeze at times, N. W.	Uniformly cloudy.	neutral	Trifling divergence in two instances only, during a few seconds.
							Scarlatina, in a mild form, is somewhat prevalent.

Day of month.	Time in hours and minutes.	Thermometer.	Barometer in inches and hundredths.	Wind, its direction and character.	Weather and modifications of clouds.	Electrical condition of atmosphere.	General Remarks.
10		60	29.40	light breezes, variable from N. to W.	Thickly clouded at great altitudes.	neutral	Scarlatina, in a mild form, is somewhat prevalent.
11		58	29.40	cold breeze N.	Gloomy, overcast sky.	neutral	
12		60	29.56	very slight, N. by E.	Hazy upwards, with little fine rain at times.	neutral	Cynanche tonsillaris, frequent among children and young persons
13		58	29.65	faint breeze N.	Thickly clouded.	neutral	
	3 p.m.			a light breeze has suddenly arisen from N.E.	Veil of cloud rapidly dispersing, — blue sky, with here and there a modification of cumulus.	positive	The electroscopes diverge freely.
14		62	29.50	zephyrs, S.E.	Fine, with various clouds.	positive	Slight divergence at times, during morning only, — distant lightning at eve.
15		62	29.70	breezes, variable from W. to N.	Manifold dense clouds, at great altitudes.	neutral	
16		62	29.75	light breezes, variable from S.E. to S. by W.	Blue sky, with modifications of cirrus.	positive	Free divergence until sunset.
17		63	29.65	breezes, S. by W.	Clear sky, and feathery cirri.	neutral	Free and continuous electric current.
	2, 30 p.m.			has varied to S. E. by E.		positive	
18				light breezes, E. by N.	Blue sky, with cirrose fibres dispersed abroad.	positive	Free divergence.
	2 p.m.	66	29.40	S. by W.	The cirrose fibres have gradually resolved into cirro cumulus.		All divergence has ceased.
	4 p.m.				Various modifications of cumulus.	neutral	
	6, 10 p.m.				Thunder approaching.	negative	Continuous discharge of sparks.
	6, 25 p.m.				Storm-cloud passing rapidly over ; — a moderate shower.	positive	Energy of electric current decreasing.
19					Heavy rains before sunrise.		
	8 a.m.				Cumulo stratus and distant nimbus.	neutral	

Day of month.	Time in hours and minutes.	Thermometer.	Barometer in inches and hundredths.	Wind, its direction and character.	Weather and modifications of clouds.	Electrical condition of atmosphere.	General Remarks.
19	2 p.m.	66	29.32	light breezes, W.	Thunder from clouds distant about a mile.	negative	Waves of electric fluid rush through the apparatus subsequent to each successive lightning-flash.
	2, 20 p.m.				Rain in abundance. Frequent showers. Densely clouded.	positive	Current of small sparks. No further trace of elec. action.
20		64	29.50	strong at times, W.S.W. decreased		neutral	
	5 p.m.				Continued gentle rain.	neutral	Numerous cases of odontalgia.
21		66	29.60	sudden squalls from W.S.W. moderate	Occasional gentle showers.	neutral	
	6 p.m.				Clear sky.	neutral	Sensation of langour and muscular relaxation very general.
22		66	29.65	mere zephyrs W.S.W. increased to smart breeze, S.W. by W.	Clouds and thin haze beneath.	neutral	
	1, 25 p.m.				Hasty dispersion of clouds, except fine cirrose fibres, here and there.	positive	Slight divergence of electroscopes immediately on dispersion of clouds; increasing for one hour, becoming uniform, and ceasing at sunset.
23		64	29.50	none	Blue sky and modifications of cirrus alternately.	neutral	Odontalgia, rheumatism, and various neuralgic affections abound.
24				none	Clear blue sky.	neutral	
	2 p.m.	66	29.40	smart breeze has just sprung up from S.E. by E. has ceased	Few modifications of cirrus cloud.	positive	Free divergence immediately on rising of the breeze.
	6 p.m.				Clouds increased — continued rain by night.		The most delicate test-instruments fail to detect any free electricity. All the last-named diseases are increased: extreme langour of the animal system prevails.
25		64	29.30	scarcely evident from S.E. breeze, S.W.	Continued gentle rain.	neutral	
	11, 30 a.m.				Fair with flocculent cirri and cirro cumulus.	neutral	
26				gale S.W.	Blue sky with cumulo floccosus.	neutral	
	2, 30 p.m.	67	29.45	slightly abated	Nimbus flying hastily over.	negative	Free divergence when a nimbus reaches zenith of apparatus.
	evening			decreased to a smart breeze	Numerous modifications of cirrus.	positive	Slight divergence steadily.

Day of month.	Time, in hours and minutes.	Thermometer. Barometer in inches and hundredths.	Wind, its direction and character.	Weather and modifications of clouds.	Electrical condition of atmosphere.	General Remarks.
27		64 29.70	strong breeze S.W.	Flocculent masses of cirrus cloud, with ragged nimbus at times, from which large rain-drops are occasionally discharged.	positive	Moderate divergence generally. Pendulums strike when a nimbus reaches zenith of apparatus. Many cases of catarrh and bronchitis occur at this time.
28	11,30 a.m.	61 29.53	gusty, S.W. constant strong breeze.	Nimbus with intervening haze. Continued fine rain and drizzling haze.	positive	Occasional slight divergence. No evidence of electric action.
29		64 29.40	strong breeze and occasional gusts, S.W.	Sudden rain squalls.	positive during rain.	Torrents of brilliant sparks incident to the rains; atmosphere neutral in the intervals.
30	6 p.m.	67 29.65	breezes, W. breeze variable.	Blue sky with cirro cumulus, &c. Gentle rains.	neutral positive	Sensation of languor and oppression, with pungent heat of the soles of the feet. Feeble current faintly visible by daylight, and appearing as a luminous brush; stream of small sparks occasionally.

The average condition of the atmosphere in this locality has been apparently highly favourable to health, throughout the month. Cases of scarlatina, cynanche, &c., which commenced about the 9th inst., assumed a mild form, and readily yielded to the usual treatment.

The various ramifications of *cirri*, and occasionally the several modifications of *cirro-cumulus*, have, especially in the fine evenings, presented several highly-beautiful pieces of sky, in which it was obvious that the great agent of the universe, electricity, was performing an active part.

During the nights of the 7th, 14th, 18th, 19th, 27th, and 30th, many small lucid meteors made their appearance, passing mostly at or near to a right angle with the magnetic meridian.

On Professor Pfaff's Theory of Galvanism. In a Letter to Professor Palmstedt, of Gottenburg, from Professor J. BERZELIUS, of Stockholm. Dated October 9th, 1841.*

Dear Friend,—I suppose that I, in my last letter to you, forgot to mention a remarkable scientific notice which I received by a letter from Professor Pfaff, of Kiel. You are aware that he has for a long time been engaged in the attempt of deciding the question, whether the contact of the metals, or the chemical effect of the liquor, be the primary action in developing the electricity of the pile. He has invented a hydro-electric pile, in which the metals are not affected by the liquor, and where no apparent chemical decomposition into its component parts is taking place, even when the phenomena of electricity are from 30 to 40 times more intense than by a pile, of equal magnitude and form, of copper, zinc, and diluted sulphuric acid. Fig. 8, plate 1, represents a horizontal section of this pile; where *a* is, an open cylinder of china covered with platinum, like those of the many a year ago used tea-pots; *b* is a cylinder of burned, porous, unglazed china, furnished with a bottom; *c* a cylinder of amalgamated zinc, open throughout; and *d* a cylindrical vessel of glazed china, or of glass. Into *b* concentrated nitric acid is to be poured, and into *d* a saturated solution of sulphate of zinc, which will here have a stronger effect than sulphuric acid diluted with one-tenth of water. The combination of the different pairs is made like that of Professor Bunson. The action was found to be continuous up to 48 hours, when he wrote to me, and no gas had issued from the surface of the zinc, nor had any oxydated zinc been precipitated in the liquor. He considered this phenomenon as an *experimentum crucis*, proving the theory of electrical contact, and he supposes that this construction of the pile may be of practical use, if the electro-magnetism is to be turned to advantage as a motive force. As to this new electrical instrument, Professor Pfaff may be mistaken in his assertion that a chemical decomposition is not taking place; because, according to all theories, it will follow, but by different phenomena from those which take place when the zinc is exposed to a free acid.

A zinc salt will arise on the metal, and, corresponding to the oxyde of the zinc, will be collected in the nitric acid; which, probably, will at length contain a great deal of nitrate of ammonia and nitrate of zinc; and the experiment will then only show that no greater corrosion of the zinc is taking place than what will be equal to the electric current.

Concerning the theoretical value of this pile, it would decide nothing, even if the question had not been decided a long time before by Professors Poggendorff and Fechner's Researches. Those

* This letter was kindly handed to us by Richard Roberts, Esq., C. E., of Manchester, who had it direct from Professor Palmstedt.—EDIT.

who attempt to decide it in the hydro-electrical way, will for ever be in a dispute, without coming to a final satisfactory conclusion; because the electrical current and chemical decomposition will always be found quite inseparable. It is alone by the dry way that an *experimentum crucis* is to be obtained, and will be obtained, in spite of the capricious phenomena of oxydation which have been ascribed to the dry way. The dry electric piles that I brought home in 1819, and which then were many years old, are still, after a lapse of twenty years that I have had them, as powerful as when I first got them. The metals which form them are leaves of tin and brass, so thin as scarcely to be measured; yet these have produced an electric action for twenty-two years, and instead of being oxydized through, are still perfectly metallic. You see, therefore, that this scientific dispute is thus carried on; that some of the persons interested in it do not lay their questions quite impartially to nature. How is it? They are looking continually for arguments in favour of an hypothesis which they consider to be the right one. They will always be found in a situation very disagreeable to the true lovers of science, not being able to be fully persuaded to the facts of the contrary party, pleading to hypothesis instead of refutations by real facts. The former party will be placed without the limits of facts, and thus they will always shew an apparent irrefragibility, although they cannot disprove the facts on the other side of the question.

This question will never be ended until the last champion of the wrong supposition has ended his earthly career; then truth will remain triumphant and uncontested.

Please to excuse me if I have too long detained you by presenting my own views on Professor Pfaff's experiment.

Your friend,

JACOB BERZELIUS.

Experimental and Theoretical Researches in Electricity, Magnetism, &c. By WILLIAM STURGEON; prior to his establishing "The Annals of Electricity, &c."

After leaving the service of the Royal Artillery, in 1820, and as my slender finances presented opportunities, I turned my attention to experimental inquiries, and to the construction of some of those philosophical apparatus which, from boyhood, I had been an admirer of, but of which, during *my* services at least, a military profession precluded all further knowledge than that which was to be learned from books of science, a species of commodity, at that time, exceedingly rare in the army, even in the distinguished branch to which I belonged.

The first piece of my apparatus which found its way into a scientific journal, was a modification of Ampere's rotating cylinders, as improved by Mr. Marsh. This apparatus was disposed of to Mr. Jones, optician, Holborn, who sent it to the United States. Mr. Jones was kind enough to draw up the following short account of it, and got it introduced to the pages of the *Philosophical Magazine* for September, 1823:—"The apparatus consists of two

sets of revolving cylinders, one suspended on each pole of an inverted horse-shoe magnet,* as represented by fig. 1, plate 3. Upon the usual insertion of the diluted nitric acid, the two sets of cylinders simultaneously enter into rotations in a very interesting and striking manner. This form of the magnet gives increased power on a reduced altitude, and the proximity of the poles materially augments the rotation of the opposed cylinders. The effect is the most pleasing we have ever seen, and was witnessed at the house of Messrs. Jones, opticians, Holborn." The appearance of this apparatus was much improved by the outer cylinders and base being made of brass.

* Prior to this apparatus making its appearance, a straight bar magnet was invariably employed to shew the action of Ampere's cylinders: the action was first shewn on one pole, and then the magnet inverted, to shew the action on the other pole.—EDIT.

Electro - Magnetical Experiments. By WILLIAM STURGEON.

To the Editors of the Philosophical Magazine and Journal.

Being desirous to understand the relation that subsists between the chemico and thermo-electric phenomena, as influenced by the magnet, so as to form a comparison of the widely different apparatus for exhibiting those phenomena; and likewise, if possible, to ascertain some general law to be observed in thus comparing the two modes of exciting this influence; the few following simple experiments suggested themselves as the most likely to give satisfaction on this point. As this investigation and comparison* seems to have escaped the attention of every other experimenter, perhaps a detail of those experiments, with their results, and a description of an instrument which I have been led to invent and construct, upon a simple principle, for the purpose of carrying on the comparison to any required extent, may not be thought uninteresting to some of your readers.

Exp. 1. I charged in the usual way, with dilute nitric acid, an Ampere's rotating cylinder, and placed it on a table. I now placed the north pole of a bar magnet on the upper edge of the outer rim of the copper part of that apparatus. I likewise imagined myself "coinciding in position with the wire about which the machine turns," and "looking towards the magnet." On bringing one of the wires of the zinc cylinder between me and the magnet, that wire was projected to the left.

Let the line ED, fig. 2, be the horizontal part of the wire in this and the following experiments, moveable on a pivot *o*; then, when the wire is said to be projected to the right, it is meant that the point E is projected towards R, as *o* R; and when to the left, the point E is projected towards L, as *o* L.

Exp. 2. I now presented the south pole of the magnet to the wire. The latter was projected to the right.

Observation. This chemico-galvanized wire was evidently the

* There may be said to be one objection; for a comparison has certainly been made by a philosopher of acknowledged skill; and it is with due deference that I cannot subscribe to the conclusions of that gentleman.

ascending wire from the zinc to the copper; or (which is the same thing) the descending wire from the copper to the zinc.

Exp. 3. I now suspended, by a piece of untwisted silk, a semicircular copper wire, with a zinc diameter, as in fig. 3; *ccc* is a fine copper wire; and *zzz* a fine slip of zinc, soldered to the former at the extremities *zz*. The north pole of the magnet was now placed close to one arm of the semicircular arc, as shown in the figure, and the lamp applied at *E*. The wire was projected to the left.

Exp. 4. The semicircle adjusted as before, the lamp was now applied at *D*. The wire was projected to the right.

Exp. 5 and 6. In these experiments the lamp was applied as before, but the south pole of the magnet was now presented to the wire. The motions were the reverse of the two former experiments.

I have tried rectangles instead of semicircles, but there is no difference in the results.

If a semicircle or rectangle be made of platinum and silver wires, the former supplying the place of the copper, and the latter be substituted for the zinc, in the above thermo experiments, the results of the two machines are exactly alike; hence the copper and platinum wires are possessed of the same kind of electricity, and so are the zinc and silver each to each, but of the contrary kind of the other two wires: so if the copper be positive to the zinc, by this magnetic test, we must necessarily conclude that the platinum is positive to the silver.

But this is not all that is to be observed in these experiments; we must compare the results of the chemico with those of the thermo experiments, and endeavour to trace the relation that subsists in the phenomena exhibited by those two modes of exciting the electrical influence.

We will therefore compare our chemico experiment 1, with our thermo experiment 3. We here find the wire projected to the *left* in both cases.

We will now suppose the immersed plates of the chemico apparatus to correspond with the heated union of the thermo wires, and the most remote extremities of the chemico conducting wires with the coldest part of the thermo apparatus. It will follow, that as the copper wire (by the supposition) is as evidently ascending in the thermo machine, as the zinc wire is ascending in the chemico cylinder; and as they are both propelled in the same direction (left), by the approximation of the north pole of the magnet, the forces of the two galvanised wires must of necessity be exerted in contrary directions. For although they are both propelled the same way yet they are of contrary kinds or names.

Again. Let us now suppose the immersed plates of the chemico apparatus to correspond with the coldest union of the thermo wires, and the most remote extremities of the chemico conducting wires with the heated junction of the thermo machine. In this case, the two wires nearest the magnet of the chemico and thermo machines will in all respects correspond with each other; for they are now both descending from the copper, or both ascending from the zinc

(by the supposition). Hence the forces of the two galvanised wires are exerted in the same direction; for the wires are of the same kind, and are propelled the same way.

The conclusion still holds good, when the junction of the wires is heated at D instead of E.

Having thus ascertained, by the former supposition, that the forces of the chemico and thermo galvanised wires are exerted in *contrary directions*, and that by the latter supposition these forces are exerted in the *same direction*,* I shall leave the theorizing philosopher to adopt which side of this dilemma he may think fit; for it is not my province to predict whether we are to have a thermo-electric force acting in a contrary direction to our old galvanic force, by the former supposition, or whether we are to have these two forces reconciled to each other by the latter; my business being to ascertain some general law whereby to regulate my subsequent experiments.

I have since made experiments, both chemico and thermo, with combinations of other metals, and the results have given me the greatest satisfaction that the above law is general; for the comparison is, in all that I have made, the same as with copper and zinc.†

Here follows a description of the instrument I have made, for the purpose of exhibiting and comparing chemico and thermo phenomena as influenced by the magnet; by means of which I can make the experiments with the greatest facility and exactness. I have named it the Comparing Galvanoscope.

Fig. 4. A cylindrical glass tube supported by the arm Cc of the brass stand DE; on the top of the cylinder is a moveable brass cap ff. To the upper side of this cap is adapted a thick wire, with its bobbin g and milled head H. This wire turns in two spring sockets at ii. One end of a piece of untwisted silk is fixed to this bobbin; the other end is supplied with a fine silver wire hook, which descends through a small hole in the centre of the cap into the glass cylinder. The circular plate o at the top of the stand has a flat moveable rim, which is graduated from 0 to 90° each way. This is for measuring the quantity of deflection; and hence the instrument in some measure answers the purpose of a galvanometer.

Fig. 5 shews a chemico combination out of the instrument. A is a small glass vessel containing dilute acid. The parallel slips or wires *aabb* are the dissimilar metals for the experiment. The conducting wire *ccc* is soldered to their extremities at *a* and *b*.

To make the experiment, the glass cylinder is taken off the stand by unscrewing the nut *c*, fig. 1. The combination is then hooked on and drawn to the top of the inside of the cylinder by turning the pin H. The glass vessel, with its acid, is placed on the circular plate *o*; and the cylinder replaced, and the nut screwed tight. The metals may now be let down into the fluid to any depth the experimenter thinks fit. The circle may now be adjusted by bringing *o* under one of the wires, and the magnet being applied outside of the cylinder will deflect the wire.

* Some time after this was published, I learned that Professor Cumming had previously discovered the same facts.—W. S.

† This is found not to be a general law with all metals.—W. S.

To make a thermo experiment, the glass cylinder is taken off as before, the combination hooked on, and the whole replaced; when the wire is at rest (which soon will take place, for the cylinder entirely cuts off the undulations of the external air). The graduated circle is now adjusted, and the lamp applied at L, and the magnet as in the figure.

This method of detecting the galvanic influence in delicate chemico combinations is so efficacious, that a piece of fine silver wire, such as forms a part of what is called gold lace, with a piece of zinc of the same dimensions let into the dilute acid not more than 1-10th of an inch, will cause the connecting wire (on the approach of the magnet) to be deflected 40° , sometimes 50° .

I have a great variety of combinations ready, which are placed between the leaves of a small book made for that purpose, with the names of the metals on their respective leaves.

The whole apparatus packs up in a neat mahogany box.

This instrument differs from the galvanometers I have seen described. I use a powerful magnet for detecting and determining a slight galvanic influence; whereas in the galvanometer a feeble needle is used for that purpose. Many other differences might easily be pointed out, whether advantages or not is not for me to determine.

I am, gentlemen, yours, &c.,

Artillery Place, Woolwich, Feb., 1824.

WM. STURGEON.

Electro and Thermo-Magnetical Experiments. By WM. STURGEON.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,—Although the experiments I have detailed in my former paper have sufficiently satisfied myself with respect to the directive exertion of the forces of the differently excited wires or machines, yet it is not impossible that some may still doubt of the sufficiency of those experiments to determine the apparently anomalous phenomena: owing, perhaps they may say, to the difference of the construction of the apparatus employed for exhibiting them. I am persuaded, however, that the following mode of making the experiments will, in all probability, be sufficiently decisive to convince the most sceptical on this point.

Suspend the semi-circular copper arc, with its zinc diameter, as described in my former paper (the extremities of the metals need not be soldered, but twisted together). Wrap one of those joinings of the metals loosely with a piece of tow or unspun cotton. Dip this part of the machine into dilute nitric acid (observe, to counterpoise at the other end); present the north pole of a magnet to the same arm, and it will be projected to the right.

Take off the cotton, wash and dry the machine, and suspend it as before. Apply the lamp now instead of the acid, and the magnet as in the former experiment; that arm of the apparatus will be now propelled to the left.

I have merely pointed out this method of making the experiments, as the most likely to be understood in comparing the phenomena; but that described with the galvanoscope, for making the chemico experiment, is by far the most efficient and eligible.

The results obtained from the mode of comparing chemico and thermo phenomena, could hardly fail to suggest the idea, that chemico-excited wires would have their electrical tension increased by thermo application at the opposite extremity. And in order to try the suggestion by the test of experiment, I had recourse to the above-described simple apparatus. I twisted the copper wire a good length round the extremities of the zinc, so that as great a metallic surface as possible might be exposed to the action of the acid. After dipping the extremity wrapped with tow into the acid, I suspended the machine in the galvanoscope. On presenting the north pole of the magnet to the arm ascending from the chemico extremity, the latter was deflected to the right about 80° . From that it returned to nearly 20° : thence propelled again to nearly the same distance as before; and so vibrating several times from about 15° to 60° , when I changed the pole of the magnet. It was now deflected in a contrary direction (left) to about 70° : from thence it returned as before by the silk, endeavouring to untwist itself, and was again propelled by the magnetic influence; thus vibrating, still describing a smaller arc, and approximating nearer the magnet.

When it had become so feeble that the greatest distance did not exceed 30° , I applied the lamp at the other extremity. It was soon propelled to above 100° : and by keeping the extremity of the wires warm (it is well known that zinc would soon melt in a strong flame), I could keep it vibrating at about right angles to the pole of the magnet; for when it went further than 90° , the thermo arm became acted on by the magnetic influence, and conspiring with the reaction of the convoluted silk, the machine was frequently driven back again 40° or 50° ; but by keeping it moderately warm, it was kept at between 80° and 90° from the magnet.

I now again changed the pole of the magnet, and took away the lamp. The chemico action had now become so feeble as to be just discernible. However, by applying the lamp it soon acquired between 60° and 70° , and could be kept up to nearly the lower point.

I have repeated the experiment, with the same success, in about $6\frac{1}{2}$ minutes each time. The copper wire was about 1-60th of an inch in diameter, and the zinc about twice that thickness.

When the lamp is applied before the chemico action gets too weak, this *thermo-chemical magnetic* experiment, in miniature, is most strikingly decisive. Not having it in my power, at present, to carry on the experiment on a large scale, I am not prepared to say how it might answer. All the phenomena yet exhibited by the differently excited wires, seeming to be so perfectly analogous in every other respect than in the direction of their forces, I have, by parity of reasoning, found no difficulty in producing a thermo-rotation by the influence of a central magnet.

As this experiment seems to have baffled the exertions of some of your scientific correspondents, it may perhaps be considered of some importance in promoting the advancement of the science.

Artillery Place, Woolwich, }
Feb. 16, 1824. }

I am, yours, &c.

WM. STURGEON.

Description of a Rotative Thermo-magnetical Experiment.

By WILLIAM STURGEON.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,— Having promised, in a former paper, to communicate to your readers the method I have adopted for rotating a thermo-combination, by the influence of a central magnet, the following description of the apparatus I have constructed and employ for exhibiting the experiment, with an explanation of its management, will, I humbly hope, be sufficiently plain to be understood.

N S, in the figure 1, plate 4, is the magnet; P c P, a piece of platinum wire, bent into the form of a semi-circle, or other convenient curve; P s, P s, are two pieces of silver wire, twisted to the former at the extremities P P. The other ends of the silver wires are bent downwards at s s, and made quite sharp and smooth at the points. These points descend into the metallic cell F E, which contains pure quicksilver, with which the points communicate. A descending point c, soldered to the platinum wire, forms the pivot on which the moveable part of the machine turns. A small concavity, well polished at the bottom, is made in the point of the magnet, for the purpose of containing a small globule of mercury, and likewise for the rotating pivot to work in.

The point c being amalgamated, when it is placed in this globule of mercury, forms a communication with the magnet; and the other part of the magnet, which passes through the cell, communicates with the mercury in that cell: and the points of the silver wires being immersed in this mercury, the metallic circuit is thus rendered complete; first, through the platinum wire from P to c; thence through the pivot to the top of the magnet, and along that part of the magnet from the top to the quicksilver in the cell F E: and, lastly, along the silver wire, from the point s to the extremity at P, where it joins the platinum.

The other part of the wire machine being on the same principle as that described, the platinum arms of this apparatus, when heated by a spirit lamp or otherwise at the extremities P P, are in every respect assimilated to the arms of the rotating cylinder of Ampere; for the electric fluid is transmitted, in the same direction, through both arms of the apparatus; and hence the rotating tendency is *constant* round a central magnet; and not *impulsive*, as in other rotations with an external magnet.

The moveable part of this machine (which is the platinum and silver wires only) will rotate with a facility proportioned to the delicacy of the suspension, the difference of temperature of the parts P and c of each arm, the power of the magnet, and the dexterity of the experiments. And I must here warn the reader, that this last requisite is not the least to ensure success in the experiment; for had I not been satisfied that the apparatus was constructed upon principle, I probably might not have persevered sufficiently to attain my object. However, a slight modification of the apparatus con-

siderably facilitates the experiment, and renders it more permanent and beautiful.

A circle of lamps are placed on a stage of the same figure, in such a manner that they may coincide with the periphery of the circle, described by the points P P of the wire part of the machine, so that the latter may constantly be kept at nearly the same temperature in every part of their revolution. And the shoulder of those arms, or that part of the platinum wire to which the pivot *c* is soldered, is kept at as low a temperature as possible, by means of ether or other cooling liquid.

If, instead of lamps, a circular flame of ignited hydrogen be substituted, and regulated by a stop-cock, this part of the apparatus may perhaps be considered as its acme of perfection.

Another improvement is, by having a conducting wire from the pivot *c* to the metallic cell F E, in the same manner as the conducting wire of the copper part of M. Ampere's rotating cylinders; through the upper part of this conducting wire passes a screw with a milled head, made into the form of a cup. The pivot *c* runs in this cup, at the bottom of which is a small globule of mercury, for the better ensuring the contact. The cup is then filled up with ether, and may be supplied during the experiment in proportion to the evaporation.

The lower end of this screw rests in the hole in the top of the magnet; and by turning the milled head to the right or left, the points *ss* of the silver wires may be heightened or lowered at pleasure; and, consequently, their contact with the mercury in the cell F E may be regulated to the greatest nicety; the attainment of which was the only embarrassment I had to encounter with the original apparatus. However, by means of this improvement, my anticipations were soon agreeably realised, by witnessing the first thermo rotation ever produced by the influence of a central magnet.

I must here beg leave to observe, that the only attempt I ever heard of (and the only one perhaps on record) was with the apparatus of Professor Cumming, and a similar attempt by Professor Barlow, with a combination upon the same principles.

The latter gentleman, however, has candidly confessed the failure of the experiment, and sufficiently accounted for the inefficacy of the apparatus upon the principle of its construction.

I am, gentlemen, yours respectfully,
Artillery Place, Woolwich.

WM. STURGEON.

P. S.—April 13. I have since succeeded in forming a sphere of galvanized wires, to rotate by the influence of both poles of an internal magnet.

This experiment was suggested on reading the late Dr. Halley, on the theory of the earth; and although it may not be considered as a proof of that philosopher's notion of terrestrial magnetic variation, yet, perhaps, it may tend in some measure to strengthen the hypothesis. A description of the apparatus shall be the subject of another paper.

W. S.

THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND
CHEMISTRY;
AND
GUARDIAN OF EXPERIMENTAL SCIENCE.

FEBRUARY, 1842.

An Account of the Magnetic Observations made at the Observatory of Harvard University, Cambridge, by JOSEPH LOVERING, Hollis Professor of Mathematics and Natural Philosophy, and W. CRANCH BOND, Astronomical Observer to the College.

(Memoirs of the American Academy.)

(Continued from page 27.)

On all the preceding days observations were made during the 24 hours, so as to furnish materials for drawing as many diurnal curves. The intervals between the observations were not always five minutes as on the term-days. Thus, in May the interval was 15 minutes, and in June and July 20 minutes. Again, in August the interval was only four minutes. In August, reasons appeared for recording the thermometer at intervals of 20 minutes on all the days when the magnetometer was observed. This was sufficient to give all the regular daily thermometric changes. In October, the barometer was observed every 20 minutes during the five days. The observations on the barometer have not been repeated, as, they did not promise to facilitate the inquiry upon which we were engaged; but the thermometer has been observed regularly since the first trial every 20 minutes, whenever magnetic observations are made. The magnetic observations were conducted on all these days with the same strictness as on the term-days, at intervals such as have been denoted; 12 or 13 readings determining the place of the magnetic meridian for every required period. There may be times during every time of observation when the arc of vibration is too large to

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allow of this process, and then the 12 readings are suspended, and the extreme limit of every oscillation is recorded. The expense of the plates has precluded the idea of publishing all the diurnal curves which have been thus obtained. A specimen has been selected, which will give an impartial view of the whole investigation. On plate VI, the thermometric curves, corresponding to the five October days, may be studied by attending to the directions given on the face of it. The remarkable points are arranged in the following table:—

Abstract of the October Thermometric Curves.

Date. Civil Time.	Time of Minimum Heat.—Cam- bridge M. T.	Thermo- meter.	Time of Maximum Heat.—Cam- bridge M. T.	Thermo- meter.	Daily range of Thermo- meter.
Oct. 21	6 ^h 10' A.M.	50° 20'	1 ^h P.M.	54°	3° 40'
" 22	1 "	46	2 "	51 30'	5 30
" 23	3 50 "	34	2 "	62	28
" 24	6 50 "	40 40	2 40' "	53	12 20
" 25	1 30 "	37	11 A.M.	47 20	10 20
Means	3 52 "	41 36	1 20 P.M.	53 34	11 58
Mean curve	6 10 "	43	2 "	52 15	9 15

The absolute changes of temperature from day to day cause the difference between the means of the numbers attached to the five days and the mean curve, as in the two last lines of the table. The mean curve is drawn by adding together the figures observed at the same time on the several days, and dividing by five. The result is the mean temperature for that period of the day. Then the times of maximum and minimum of the mean curve are found from the greatest and least values of these mean results. The absolute variations of temperature from one day to another vitiate to a certain extent this process. The same method is pursued for laying down the mean diurnal magnetic curves; the absolute changes of magnetic declination from one day to another are too inconsiderable to produce any great error in the result. But the mean of the daily magnetic curves is materially injured by the *irregular* perturbations of the magnetic meridian. Hence, it is supposed that the mean thermometric curves have a fair comparison with the mean magnetic curves. For the sake of facilitating this comparison, the following abstract

is given of the single and mean diurnal magnetic curves which are drawn at length on plate IV :—

Date. Civil Time.	Time of Mini- mum Declination Cambridge M.T.	Readings of the scale.	Time of Maxi- mum Declination Cambridge M.T.	Readings of the scale.	Daily Range.
October 21	8 ^h 6' A.M.	102.562	0 ^h 28' P.M.	92.354	10.208
" 22	8 36 "	104.333	11 46 A.M.	93.364	10.969
" 23	9 11 "	103.375	2 51 P.M.	96.031	7.344
" 24	8 16 "	103.208	2 31 "	95.396	7.812
" 25	5 11 "	101.740	1 41 "	92.302	9.438
Means	7 52 "	103.044	1 27 "	93.889	9.154
Mean curve	8 36 "	102.250	3 1 "	94.368	7.882

We have now the following results of comparison :—

Times of maximum temperature range from 11^h A.M. to 2^h 40' P.M. = 3^h 40'.
 " " declination " " 11 46' " to 2 51 " = 3 5.
 " of minimum temperature " " 1 " to 6 50 A.M. = 5 50.
 " " declination " " 5 11 " to 9 11 " = 4 hrs.

It appears, then, that the times of maximum and minimum magnetic declination are comprised within narrower limits than those of the greatest and least temperature ; and yet the last phenomena, as every one believes, depend on the apparent daily motion of the sun. It is to be observed that these days were selected and the curves published before the comparison was made. They are to be regarded as a fair index of the whole series of observations of a similar character taken in each month since August 1840, inclusive, when the thermometric and magnetic changes were first observed in connexion. As far as any dependence can be placed upon them, they authorize us in stating that the diurnal magnetic curve is a more exact and definite function of solar time than the regular daily change of temperature. In whatever particular the comparison is made, the preference attaches to the magnetic curves. We have seen how it is in regard to the *limits* of the time of maximum and minimum. The greatest variation of any daily range of the thermometer, from the mean of the ranges, is more than $\frac{17}{12}$ of the mean range. The greatest variation of any magnetic range from the mean of the magnetic range is less than $\frac{1}{2}$ of the mean of the magnetic ranges. The most observable deviations from mean results in the magnetic observations pertain to the times of maximum declination of October 21-2 and 22-3, and the times of minimum declination of October 25-6. If they were excluded, the limits of the times of

maximum declination would be reduced from 3 hours to 1 hour and 10 minutes, and of minimum declination from 4 hours to 1^h 5'. In regard to the first of these days, an unusual and irregular derangement of the magnetic equilibrium occurs between 1^h and 3^h, P.M., Cambridge M.T.; and there is reason to think that the maximum declination, if the extraordinary influence could be eliminated, would fall between these disturbed hours. There is even a doubt whether now the western excursion at 3 P.M. ought not to be taken as the western limit of that day. The irregular perturbations, which do not generally begin before 7 or 8 o'clock in the evening, commenced their operations on this day earlier than usual by many hours. We are confirmed in this opinion by examining the curve of the next day from 10 o'clock Gott. M.T.* This curve is a continuation of the first from the place where it ends at the right hand of the plate; and we see from it that the perturbations continue during the whole night and morning till nearly the time of the next minimum, one digression of the magnetometer within 30 minutes, about an hour before midnight of October 21st, amounting to 20'. These agitations are felt at intervals till the following midnight, and may be allowed any influence that is thought justly attributable to them in hastening the time of maximum of the second day. It is admitted that this maximum appears satisfactorily formed on the curve, so as to leave no doubt that the time selected for it, 11^h 46' A.M., Cambridge M.T., is correct. And further, in regard to the minimum of the last of the five days, there may be a question what precise moment between 5 and 7 o'clock A.M., Cambridge M.T., should be chosen. It seems probable that the minimum should be formed earlier than usual on this day, as the maximum occurs later than the average time; so that the whole curve from the point of minimum to that of maximum takes a wider sweep of time than usual; a glance at the plate makes this clear. These explanatory suggestions as to the extreme cases, if valid, will give still greater pre-eminence to the diurnal magnetic curves over the contemporaneous thermometric curves, so far as relates to the present comparison. But it is not at all necessary for our purpose to press this apology. No abatement, for similar reasons, is required in regard to the thermometric curves. Although occasional tempests of heat and cold occur, they cannot compete with the irregular disturbances of the magnetic force, where a change of 20' occurs in half an hour, while the greatest range from minimum to maximum on any of these days is only 11'. There is safety, then, in affirming that the changes in the declination of a magnetised bar would be a better index of solar time than a standard thermometer; or again, that starting from a known hour of the day, with a given declination, we might venture a closer prediction, founded on calculation, as to the position of the same bar six hours afterwards, than

* The commencement of the magnetic day is in all cases at 10 o'clock P.M. of mean Gottingen time.

would be safe with a thermometer under like conditions ; care being taken of course to select that portion of the day which is most free from *extraordinary* magnetic and thermometric changes.

Let us now see how this conclusion is sustained by more extensive observations. To this end, tables are presented of the monthly means, comprising the mean periods of magnetic and thermometric minima and maxima since this kind of observation was instituted at the Cambridge Observatory :—

Thermometric Table.

Months.	Number of days observed.	Time of the Mean Minimum. Cambridge M.T.	Value of the Mean Minimum	Time of the Mean Maximum. Cambridge M.T.	Value of the Mean Maximum	Mean Daily Range.
August	5	5 ^h 56' A.M.	58° 36'	2 ^h 36' P.M.	77° 48'	19° 12'
September	5	5 56 "	41	2 16 "	63 48	22 48
October	5	6 16 "	43	2 06 "	52 06	9 06
November	3	5 36 "	25 48	2 56 "	35 20	9 32
December*	3		16 40	0 36 "	29 50	13 10
January	3	6 36 "	24 20	2 36 "	33 30	9 10
February	3	5 56 "	20 20	2 16 "	39 20	19 00
Means		5 53 "	32 49	2 12 "	47 23	14 34

Magnetic Table.

Months.	Number of days observed.	Time of Mean Minimum Declination. Cambridge M. T.	Reading of the Scale for Mean Minimum.	Time of Mean Maximum Declination. Cambridge M.T.	Reading of the Scale for Mean Maximum.	Mean Daily Range.
May	3	6 ^h 26' A.M.	114.523	1 ^h 06' P. M.	100.208	14.315
June	10	6 56 "	110.528	1 56 "	100.875	9.653
August	5	6 16 "	126.564	1 44 "	115.135	11.429
September†	5		111.925	11 36 A. M.	96.452	15.473
October	5	8 56 "	102.250	3 1 P. M.	94.368	7.882
November	3	8 51 "	111.958	1 16 "	95.927	16.031
December	3	8 06 "	100.767	3 16 "	92.226	8.541
January	3	9 16 "	98.871	3 26 "	90.062	8.809
February	3	8 16 "	99.816	3 16 "	91.090	8.726
Means		7 53 "	108.578	2 04 "	97.371	11.206

* Time of minimum for this month is 6^h 26' P.M., Cambridge M.T. From this time the thermometer rises during 18 hours, till the period of maximum.

† The time of mean minimum for September is by the observations 8^h 13' P. M., Cambridge M.T. From this time the western deviation increases till 11^h 36' A.M., the time of maximum declination.

The times of the monthly mean minima of temperature for the seven months from August inclusive, December being rejected as anomalous, range between $4^h 56'$ and $6^h 36'$ A.M., so as to be all comprised in the space of $1^h 40'$. Similar times for the maxima points are included within $2^h 20'$ from $0^h 36'$ to $2^h 56'$ P.M. It also appears that the times of the monthly means of minimum declination for the same months, together with May and June, if we leave out September, come between the limits of $6^h 16'$ and $9^h 16'$, or an interval of 3 hours, while the times of mean maximum declination, if we exclude September, are confined within the limits of $1^h 06'$ and $3^h 26'$ or $2^h 20'$. All circumstances being considered, these results are favourable to the theory which assigns to the daily changes of magnetic declination as precise a dependence on solar time as can be claimed for the corresponding variations of temperature. As far as coincidence of time between two phenomena proves one to be the cause and the other the effect, the daily oscillations of the magnetic meridian are as clearly referable to the sun's agency as the familiar rise and fall of local temperature during the same period. It should be considered, that in the instance of magnetism the limits are estimated for nine months, while those of the thermometer extend over only seven months. The longer period affords, of course, a wider field for the display of extreme cases. The disadvantage to magnetism is increased by the influence which the remarkable fluctuations of magnetic influence exert upon the regular periodic phases. As observations on temperature are hereafter to be made parallel with those of magnetic declination, we shall be able, at some future time, to present the results of a larger and more satisfactory comparison of the two sets of changes than our present materials can afford. The limits for the times of maximum declination and temperature are the same; the limits of minimum declination are greater than those of minimum temperature. The considerations which have led to the exclusion of some of the observations must now be stated. First, we consider the mean thermometric curve for December. The maximum which falls 36 minutes after noon is earlier than usual; while the minimum is 12 hours from the average time of greatest cold, and would come at $6^h 26'$ P.M., instead of the morning. We suppose, therefore, the true time of maximum for this month to be merged under irregular changes, so as to escape notice even in the mean result. This mean was derived from three days' observations. The minimum for one of the days happened at $5^h 56'$ A.M., for another at $2^h 14'$; while on the third day the changes of temperature were so frequent and disorderly, from alterations in the wind, and sudden variations from a clear to an overcast sky, that the lowest temperature is at $6^h 14'$ P.M., and the highest at $6^h 56'$ A.M. The eccentricities of this day are sufficient to vitiate the whole result, and prevent the real time of minimum from appearing even in the mean of several days. To guard against this source of error, it is important that the monthly means should

be deduced from as large a number of days as can be conveniently observed ; every day in the month would not be superfluous. This could easily be done at Cambridge, if provision were made for conducting the two hourly observations on every day according to the English plan. But these daily observations of the magnetometer have not been attempted, and would be impossible with the present resources of the Observatory.* It is not surprising that what occurred once in six months' observations on the temperature, should have exhibited itself also in the diurnal curves of the declination magnetometer. This brings up the case of the September magnetic curve, which we have excluded from any part in determining the mean quantities, because it was calculated to injure the just average values which we are endeavouring to reach. The curve in question may be traced out on Plate V, by following the directions there given, and a single glance will show how the times of its extreme elongations differ from those of the other three mean curves, which are on the same plate. The general appearance of these three curves indicates a law to which the fourth curve must be a palpable exception and transgression. An attention to the separate diurnal curves for the five days in September, from which the mean curve is reduced, will afford an explanation of this violation of what we may regard a principle of the earth's magnetism. On September 20-1, (magnetic day, commencing, as in all cases, at 10 P.M., Gott. M.T.,) we have these three maxima :

				Readings of the Scale.
1st at 11 ^h 36'	P.M., Cambridge	M.T., September	20	= 82'.5.
2d " 4 56	A.M., "	" "	21	= 79.5.
3d " 11 56	A.M., "	" "	21	= 88.5.

and these eight minima :

				Readings of the Scale.
1st at 3 ^h 14'	P.M., of September	20	= 112.	
2d " 5 46	" "	"	= 116.	
3d " 7 56	" "	"	= 123.	
				Reading at 8 ^h 16' P.M., 109.
4th " 8 26	" "	"	= 125.	
5th " 0 46	A.M. "	21	= 116.	
				Reading at 1 ^h 06' A.M., 98.5.
6th " 1 54	" "	"	= 117.	
7th " 6 06	" "	"	= 115.5.	
8th " 2 31	P.M. "	"	= 108.	

The whole range of the magnetometer during this day is 45'.5. The smallest elongation from the meridian is at 8^h 26' P.M., and the greatest at 4^h 56' A.M., as the lowest number of the scale indicates greatest declination. But these, like the rest of the maxima and minima of this day, are mere lawless excursions caused by sudden derangements, and have no connexion with the regular diurnal

* A Director, with three assistants, all of whose time should be devoted to the work, have been considered, elsewhere, as the full *personnel* of such an establishment.

magnetic curve. Between the third and fourth minima the bar moves to $109'$, or $14'$ in arc, in the space of 20 minutes, and then falls back eastward again. So between the fifth and sixth minima the bar moves to $98'.5$ of the scale, or $18'$ forward, and back again in $1^h 8'$. The time occupied in going from the second or greatest maximum to the seventh minimum is only $1^h 10'$, although the space is $36'$, or three times the ordinary daily range of the magnetic meridian for this month. If these perturbations were less frequent and not spread over the whole 24 hours, means might be devised of shutting out their influence, and interpolating the true daily curve in the gap left by their removal. But on this day no safe way of making the reduction presents itself, and the times of maximum and minimum are both left indeterminate.

On September 22-3, the magnetometer was more quiet; occasional oscillations of considerable extent occur, but the maximum point is very regularly formed at $11^h 36'$ A.M., and the minimum, though less definite, is placed at $6^h 06'$ A.M. On September 24th, westerly disturbances take place between $4^h 36'$ and $7^h 36'$ A.M., the time when the minimum generally shows itself; the effect is to bring the mean curve at this period too far to the west, or to make the apparent mean time of maximum earlier than it is in fact, or would appear if the observations were free from irregular variations. These derangements do not cease till nearly the close of the third magnetic day, so as to throw uncertainty on the time of maximum of this day also.

September 24-5, (Magnetic day.) Disturbances break out again in strange forms. This day was affected by wholly unprecedented motions; distinguished not so much for their extent as for their number and the rapidity with which they succeeded one another. A faint idea of them is conveyed by looking at those which occurred between $7^h 20'$ A.M., and $7^h 40'$ A.M., Gott. M.T., of October 22nd, represented on plate IV. The direction of the motion changed 60 times or more, from east to west and back, between 1^h and 2^h P.M., Gott. M.T. The whole sweep of the needle for this day is only $26'$, an area much less than is often traversed in the diurnal motion; but the number and frequency of the oscillations is unparalleled. The greatest declination occurs at $3^h 60'$ A.M. Cambridge M.T., and the least at $3^h 36'$ P.M.; but another minimum, more nearly resembling the regular daily minimum, appears at $5^h 36'$ A.M.

September 25-6, (Magnetic day.) The magnetic storm has subsided. The curve for this day is as quiet as that of the 4th day on Plate IV. The eye readily perceives, that now we have only regular diurnal changes, and that the times of greatest and least elongation from the astronomical meridian can be trusted. The minimum is at $6^h 26'$ A.M., and the maximum at $0^h 16'$ P.M., Cambridge M.T.

We have here ample and abundant explanation of that singular figure assumed by the mean daily curve for September. We look

for the time when some measure shall be devised for evading the errors which such extraordinary changes of the magnetic declination entail on the mean values of the regular variations. If hereafter the dependence of the magnetic declination upon the hour of the solar day shall be so accurately discovered as to be reduced to a formula, we may be able, by the help of that portion of the curve which is undisturbed, to calculate the remainder. At present this formula must be an empirical one, derived from the faulty observations themselves, and in its defective state is available only in a partial degree, for purifying these observations. Our chief resource now lies in levelling, as far as possible, the excessive excursions by the influence of undisturbed days with which they are combined; though this can be done only by sacrificing, in part, the more perfect observations. The case in hand teaches us that this method will not always be effectual in bringing out approximate results. The irregularities may be so great as to over-rule the regular law. This is less likely to happen in proportion to the number of days that can be observed in each month; and hence, again, the necessity of deducing our means from as numerous observations as can be obtained.

The dependence of the diurnal magnetic changes on solar time rests upon the evidence of a large number of observations, collected from remote sources. But there is a difficulty in conceiving of the exact manner in which this connection is sustained. Perhaps it will always be a hopeless task to attempt to trace the intricate path by which the heat deposited at one moment in the centre of our system, arrives at its final result of causing a deviation in the direction of the magnetic meridian. And while this is the case, it will be impossible to enter upon the mathematical analysis of the problem, and deduce formula which can be used for detecting the errors of theory, or correcting or supplying the deficiencies of observation, according to the well-known relation subsisting between these different methods of investigation. But the artifices of analysis will frequently take hold of cases which cannot be approached by any direct process. The observations allow us to proceed upon the ground that the declination, or the ordinate of the diurnal curve of declination, is a function of the solar day. It may, then, like any other periodic function, be supposed to be expressed in a series of terms, arranged according to the sines and cosines of the time and its integral multiples.* Thus, if

t = the time expressed in parts of a day as its unit,
 d = the ordinate of the diurnal curve for the time t ,
 n = the ratio of the circumference to the diameter,
 n = any integer whatever;

* It was according to this mathematical developement that Professor Peirce calculated the empirical curves.

and if S denote the sum of the terms which correspond to the different values of n , we have for the general form :

$$D = A + S.C_n \sin. 2 \pi n (t + c_n).$$

The values of A , C_n and c_n are readily determined by the following formula. Let observations be taken at equal intervals for several whole days, and let

h = time of observation, counted from the beginning of each magnetic day, in parts of a day as unity :

D^h = the mean of the observations taken at the time h of each day.

Then, if S' denote the sum of all the terms which correspond to the different values of h , we have

$$1. m A = S' D_h$$

m representing the number of intervals on each day.

$$2. m C_n \cos. 2 \pi n c_n = 2 S' D_h \sin. 2 \pi n h.$$

$$3. m C_n \sin. 2 \pi n c_n = 2 S' D_h \cos. 2 \pi n h.$$

There is no known periodic function which does not admit of developement according to the sines and cosines of the time and its integral multiples, and, in the absence of positive evidence, the same thing may be assumed in regard to that under present consideration.

The constant A , being equal to $\frac{S D_h}{m}$, is the mean of all the partial results obtained from observation for the several intervals into which the day is distributed for this purpose. By substituting different values for n , we obtain an indefinite number of terms out of the general one $C_n \sin. 2 \pi n (t + c_n)$. It appears, however, from the calculation, that the series rapidly converges, so that the first four or five terms are sufficient to give the declination within a degree of exactness corresponding to the accuracy of the observations themselves. Dividing the 2nd equation by the 3rd, we have the value of the tang. $2 \pi n c_n$, and multiplying equation 2nd by $\cos. 2 \pi n c_n$, and equation 3rd by $\sin. 2 \pi n c_n$, and adding them together, we readily find the value of C_n . Thus, if the numbers 1, 2, 3, be successively taken for n , we shall have the following equation for finding the approximate declination, or the empirical magnetic curve:—

$$D = A + C_1 \sin. 2 \pi (t + c_1) + C_2 \sin. 4 \pi (t + c_2) + C_3 \sin. 6 \pi (t + c_3).$$

The empirical thermometric curve is calculated on the same principle by this formula:—

$$D = B + D_1 \sin. 2 \pi (t + d_1) + D_2 \sin. 4 \pi (t + d_2) + D_3 \sin. 6 \pi (t + d_3).$$

Plates IV, and VI, will show how rapidly the series of both formulæ converge, and the limit of error incurred by dropping all the terms after the 5th. In the formulæ for October, the 5th term of the declination cannot exceed ,034 of a minute, and the 5th term in the value of the temperature cannot be greater than ,4 of a degree of Fahrenheit. From the nature of an empirical curve, our confidence in it must bear some proportion to the accuracy of the observations. If these observations are exposed to errors from any cause, as we have seen that they are, the empirical curve will suffer, though in a less degree, on their account. The error which in a single diurnal curve is left in its naked state, is of course diminished in the mean curve of several days by the levelling influence which all the days exercise upon any single one. But this process reduces: it does not extinguish the error. The passage from the mean of the observed curves to the empirical curve, carries us one step further towards the true expression of the actual phenomena of magnetism. For a considerable mean error arising from irregular disturbances, which in the first is concentrated upon a single moment, will be in the second curve distributed over the whole day, and may therefore disfigure the general character of the day, though it does not distort extremely any particular part. Moreover, it is easy, in calculating the values of the constants in the empirical formula, to omit observations of an extraordinary character, and which are notoriously burdened with strange anomalies. This we see on Plate V, in the instance of the September days, and to a less extent in October. The whole character of the curve for the former is changed from what we have reason to believe is the real diurnal curve; although it has escaped those large and prominent excursions which appear three or four times in the mean of the observed curves. In seasons of great disturbance it would be more safe to rely on the empirical curve than the observed curve; but in quiet times, as the empirical curve borrows all its truth and expression from these observations, the latter have more claim to consideration than the calculated places. It is obvious, from the principle on which the empirical curve rests, and the manner in which the constants are deduced, that they will answer only for one curve, and must be calculated separately for every new curve that is required. As the form of these equations, and the time, which is the only variable, are the same for each curve, whatever changes exist in the diurnal curve from one month to another in the year, must be indicated by a corresponding change in the independent constants. And moreover, if there be, as the comparison of recent and old observations lead us to believe, secular periods for the magnetic declination, they will betray themselves by slow variations in the mean yearly values of these same constants. It becomes, then, an object of curious inquiry to ascertain what are the values of A , C_1 , C_2 , C_3 ; c_1 , c_2 , c_3 , &c., for every month in the year; and after this their mean values from one year to another. It is possible that

the laws of the secular changes may be better studied from the variations of these constants than from immediate observations. Four of these formulæ are here given, with the names of the months to which they belong, and the number of days employed in calculating them; t = the time from 0^h Gott. m. t.

June, 10 days. Declination*

$$= 90^{\circ}17',3 - 3',853 \sin.(t - 16^{\text{h}}21^{\text{m}}24^{\text{s}}) - 1',537 \sin. 2(t - 9^{\text{h}}31^{\text{m}}24^{\text{s}}) \\ - 0',948 \sin. 3(t + 0^{\text{h}}27^{\text{m}}9^{\text{s}}) - 0',644 \sin. 4(t - 4^{\text{h}}22^{\text{m}}9^{\text{s}}).$$

August, 4 days. Declination

$$= 9^{\circ}13',9 - 3,907 \sin.(t - 15^{\text{h}}12^{\text{m}}47^{\text{s}}) - 2',009 \sin. 2(t - 9^{\text{h}}46^{\text{m}}58^{\text{s}}) - \\ 0',878 \sin. 3(t + 0^{\text{h}}51^{\text{m}}15^{\text{s}}).$$

September, 5 days. Declination

$$= 9^{\circ}21',9 - 2',932 \sin.(t - 9^{\text{h}}34^{\text{m}}18^{\text{s}}) - 1',530 \sin. 2(t - 8^{\text{h}}12^{\text{m}}8^{\text{s}}) - \\ 0',494 \sin. 3(t + 1^{\text{h}}32^{\text{m}}29^{\text{s}}) - 1',090 \sin. 4(t - 0^{\text{h}}29^{\text{m}}58^{\text{s}}).$$

October, 5 days. Declination

$$= 9^{\circ}18',7 - 1',575 \sin.(t - 13^{\text{h}}0^{\text{m}}42^{\text{s}}) - 2',379 \sin. 2(t - 10^{\text{h}}40^{\text{m}}58^{\text{s}}) - \\ 0',508 \sin. 3(t - 0^{\text{h}}4^{\text{m}}58^{\text{s}}) - 0',034 \sin. 4(t + 0^{\text{h}}12^{\text{m}}32^{\text{s}}).$$

Here we close our investigation of the diurnal magnetic curve. The existence of such a curve, regularly formed every day, cannot be doubted; its general uniformity is also very observable. The limits of the times of maximum and minimum declination in different longitudes, show conclusively that it is in some way connected with local solar time. Developing the declination according to the most general form of periodic functions, we have obtained the preceding formulæ, from which the empirical curves drawn on Plate V were calculated. These calculated curves stand there side by side with the mean observed curves, by which the constants of the formulæ were determined. The calculated curve, as we might expect, is less broken than the mean curve; still, the two agree in a striking manner, and the greatest deviations are in those months which suffered most from magnetic perturbations. In June and August the empirical curve and the mean curve keep close together, and these were periods of unusual magnetic repose; for in the mean of the latter month the disturbed term day was omitted. June was most quiet of the two, and shows it by a superior agreement between its mean and empirical curve. If there were no permanent change of declination, but only the daily oscillation uninterrupted by disorderly fluctuations, the meridian would swing, day after day, through the same arc; and a few observations would be sufficient to establish a rigorous formula, which would evolve an

* The first term in the value of the declination is obtained directly in parts of the scale, and is afterwards reduced to absolute numbers in the usual way of deriving the real declination from the reading of the scale. This process will be soon explained.

empirical curve strictly coincident with the observed curve. The want of this uniformity is felt in the variation of the constants of the formulæ already given. This simplicity does not exist in the motions of the heavenly bodies any more than in the magnetic movements. But the analysis is different. In astronomy we know the cause of the disturbance, and allow for it at once, without deranging the general analytical expression. In the other case we have no theory, no hypothesis; and the mathematical form must vary with the observations. Hence the difference between the constants in the formulæ for the four months. They are no greater than might be expected from the known change of absolute declination from day to day, the limits of the times of maxima and minima, and the longer and more irregular derangements which beset the diurnal movement. The mean curves of many months, drawn from the most abundant materials, are requisite for investigating the law by which these constants vary, and rendering them available for calculating the secular periods of the earth's magnetism. We think it is apparent from all that has been adduced, that the diurnal magnetic curve is as clearly a function of solar time as the daily thermometric curve. We are not to expect any greater uniformity in the effect than in the cause. If the thermometric curve is sometimes imperfectly formed, the same thing may happen to the magnetic curve without destroying our belief in its connexion with the sun. The change of constants in one class of formulæ appears likewise in the other, as the three following thermometric formulæ make manifest:—

August, 5 days. Temperature

$$= 67^{\circ},6 + 8^{\circ},8 \sin. (t - 15^h 57^m 28^s) + 0^{\circ},9 \sin. 2 (t - 4^h 34^m 56^s) + 1^{\circ},1 \sin. 3 (t + 0^h 44^m 56^s).$$

September, 5 days. Temperature

$$= 50^{\circ},2 - 10^{\circ},0 \sin. (t - 13^h 38^m 54^s) - 3^{\circ},2 \sin. 2 (t - 9^h 31^m 4^s) - 0^{\circ},3 \sin. 3 (t - 2^h) - 0^{\circ},8 \sin. 4 (t - 4^h 39^m 48^s).$$

October, 5 days. Temperature

$$= 47^{\circ},7 - 3^{\circ},8 \sin. (t - 14^h 21^m 44^s) - 0^{\circ},8 \sin. 2 (t - 9^h 17^m 36^s) - 0^{\circ},4 \sin. 3 (t - 6^h 35^m 25^s) - 0^{\circ},4 \sin. 4 (t - 0^h 38^m 12^s).$$

It still remains to discuss briefly those disturbances of the magnetic meridian which have no apparent law. We have occasionally alluded to them as irregular perturbations which produce perplexity in ascertaining the true diurnal curve. We are to inquire whether even they must be regarded as wholly inexplicable, or whether they cannot be connected in coincidence of time at least with other well-known phenomena of nature. There are few days in the year when strange fluctuations of greater or less amount are not exhibited; but there are some periods distinguished above all others by

their remarkable frequency and magnitude. We annex a brief history of each month in this respect.

1840.

March 27-8. No perturbations of importance; only two unusual excursions, one at 2^h 45', A. M. Gott. M. T. (March 28), and the other at 4^h 15', A. M.

April. The observations of this month are very defective.

May 29-30. Term-day. Irregular disturbances of large amount, from 11 o'clock, P. M. Gott. M. T. (May 29) to 12^h of May 30. The whole sweep of the instrument through the day is 57'.2; and once, between 3^h 50' A. M. and 4^h 10, the declination changes 47'.2 in 11^m, or four times the average daily swing from maximum to minimum. The other days observed in May were not distinguished above the average by perturbations.

June. The ten days observed in this month were all unusually quiet.

July 24-5. Between 6 and 8 o'clock, P. M. Gott. M. T. (July 25) the arc of vibration of the magnetometer amounted to 20', so as to require the substitution of its extreme limits instead of the 12 readings at intervals of 10 seconds. But this large movement was not accompanied by any considerable change of absolute declination, and the whole magnetic day was undisturbed.

August. The Term-day of Aug. 28-9 was greatly deranged from 10^h 40' P. M. Gott. M. T. to 8^h A. M., and small perturbations were experienced for four or five hours after this time. The whole range of the magnetic declination amounted to 61'. Once between 6^h 40' and 7^h 20' A. M. the change of declination exceeded 43' in 27 minutes. The other days observed in August were still.

September 21-2. The sweep during this day was 45'.5. The disturbances on the days observed in this month have been already discussed at length. They were distinguished more by number than extent of arc.

October. The October curves were not entirely free from disturbances, though they are all comprehended, in their widest excursions, in a zone of 22' in breadth.

November. The three days of this month have some perturbations, but none deserving especial notice.

December. Two of the curves observed in this month were disturbed considerably; yet the whole range does not surpass 28'.5.

1841.

January. The curves observed in this month were generally regular. On the 26th of January, between 4^h and 5^h A. M. Gott. M. T., a small disturbance was felt amounting to 17'.

February. General perturbations spread over the three days of this month, particularly observed at night. Those of greatest extent occurred on the term day, February 26-7, amounting in one case to 16' of arc in 15 minutes of time. In addition to these

extracts from the records of the different months, Table II, contains a column showing the extremes of the magnetometer every day when complete observations were made.

The theory of the aurora borealis, which has of late years found most favour with men of science, supposes it to have some connexion with electricity and magnetism. It is important to investigate this subject further, and see whether there be any and what relation between this brilliant appearance of the heavens and the derangements of the magnetic declination. For this purpose a careful record has been kept of all the auroral appearances that have been noticed at the Cambridge Observatory; some of the most remarkable presented themselves at times when the regular observations on the declination magnetometer were in progress, and pains have been taken, whenever it was practicable, to watch the instrument on all other occasions when the heavens gave signs of preparation for such an exhibition. Annexed is a list of those which were displayed on a grand scale.

April 24-5. Slight aurora.

May 28-9. Remarkable aurora. An arch was formed, at 2^h 39', A. M. Gott. M. T., running, as nearly as could be ascertained, at right angles to the magnetic meridian. A crown began to form at 4^h 24'. Its position was referred to α Cor. Borealis, which was then on the meridian. As it was nearly at the same altitude of 74° 52', and to the west of the meridian, it could not have been far from the magnetic pole. Shortly after this the arch was broken up, and the northern sky covered with pulsations of light.

May 29-30. Brilliant aurora. The auroral arch was first seen at 2^h 32' A. M. Gott. M. T., extending from a point nearly east, to within a few degrees of the western horizon. The light was intense. The apex of the arch was situated 28° at first, and at 2^h 42', 30° south of the zenith. After this time, the light became broken and scattered, flying from east to west. This arch was entirely detached from the main body of the aurora, and resembled a streamer. In the north there was a diffused light, but very bright; and swift flashes towards the zenith. At 3^h 59' a large meteor was seen in the north, 20° high, descending towards the north-west.

June 26-7. About 2^h 36' A. M. Gott. M. T., an aurora was seen at the north, of a white diffused light. At 6^h 20', the aurora assumed a dull appearance, with dark wane intermixed. At 7^h A. M. the aurora became more active, and some streamers were seen. The needle was slightly affected at this time.

July 4-5. Between 5^h 28' and 5^h 34' Gott. M. T., bright diffused northern lights; occasionally long streamers; wane clouds near the northern horizon; magnetometer quiet.

July 29-30. At 2^h 45' A. M. Gott. M. T. (July 30), an auroral arch was formed 7° above the horizon, and very still. At 9^h 40' it began its motion up towards the zenith; rose to the altitude of

30°. At 3^h 54' the aurora had ceased. The light was dull during the whole time.

August 19-20. A steady auroral arch was observed. It was double, and the altitude of its apex at 3^h 54' A.M. Gott. M.T. (August 20) was 7° or 8°; its colour was dull white. This aurora continued till 4^h, and at 4^h 11' the northern lights had entirely disappeared.

August 28-9. An auroral arch appeared running from east to west, of intense brightness and diffused, but without streamers. Apex nearly on the meridian, and altitude 45° at 2^h 30' A.M. Gott. M.T. At 3^h 20' streamers shot up 60° from the horizon; the altitude of its highest part was about 76° 43', as found from its place among the stars.

October 22-3. Between 1^h and 2^h A.M. Gott. M.T., an aurora of a steady blue light was first perceived; it afterwards became brighter and whiter; the altitude was 3°; wane clouds below. At 5^h 20', the aurora was low.

November 30. At 3^h 24' A.M. Gott. M.T., an aurora was seen of white diffused light. No regular arch was formed. The magnetometer was quiet.

It appears, from this abstract of the records, that the days most distinguished for auroral appearances, are just those on which the declination of the magnetic meridian experienced the most extraordinary derangement. This was the case on May 29-30, and August 28-9. Unfortunately the magnetometer was not watched on the night of May 28-9. By referring to Plate IV, it will be seen that the declination instrument was subject to more than ordinary influences on the 22nd of October, between 0^h M. and 6^h A.M., Gott. M.T. The observers on the remarkable days of May and August describe the motions of the magnetometer as peculiar in the highest degree. It was often checked in the midst of its vibration, and suddenly forced back in the opposite direction; and this took place with such frequency at certain seasons, as to give to the motion the appearance of jerks or sharp twitches. No correspondence was noticed between the time of maximum magnetic disturbance and the formation of the auroral crown. But it was sometimes supposed, from successful comparisons in the phases of the different phenomena, that the instrument gave intimation, by some strange motion, of the most signal changes in the aurora. The display of May and August was as fine as any that has been witnessed for several years, and we should not omit to state, that on these occasions the declination magnetometer at Cambridge made the boldest sweep of the scale. As both these days happened to be term days, the opportunity was improved at other magnetic observatories of watching the coincidence between the auroral appearances and the perturbations, and the report is generally uniform from all. Plate II, which represents the May-term diurnal curve of declination, offers a specimen of the extraordinary disturbances to which we refer,

and the time of them may be compared with the phases of the aurora, which are contained in the record for that day. The excursion at g was so great, that it was found necessary to curtail it on the plate; but the extent will be readily seen from remarking that it reached to 71.4 on the scale, 47'.2 of which were traversed in 11 minutes of time. An aurora was seen on the same night at Philadelphia, New Haven, and at Toronto, U. C. A description of its appearance at New Haven may be seen in *Silliman's Journal*, No. 1, vol. xxxix.* Where facilities existed for making the observations, it was discovered to be accomplished with similar effects upon the magnetic declination as were felt at Cambridge. The magnetometer at Philadelphia experienced great derangements, although the limits were less, not exceeding 55'.8. The influence which an aurora exerts upon the earth's magnetism reaches as far and wide as the appearance itself; and probably the intensity of the effect is proportional to the brilliancy of the display. The greatest disturbance of the magnetometer at Philadelphia was, as at Cambridge, between 4^h and 5^h A. M., Gott. M. T. The deflection of the instrument at Cambridge amounted to about 57 minutes, and the extremes were separated by little more than 2 hours. Lieutenant Riddell informs us that at Toronto the arc traversed was 1° 59', which was never equalled, and approached but once on a similar occasion. We also learn from him that an aurora was noticed at Greenwich, Great Britain, on the same day; but he adds, that the disturbances there and at Toronto were very different.

Such full information is not possessed in regard to the aurora of August 28-9. It is evident, from the observations, that the magnetometer at Cambridge was more affected on that day than ever before, the whole change of declination amounting to 61'. At Toronto, where the aurora was also seen, the disturbances were equally surprising, and produced an oscillation of 1° 33' in declination. The greatest amount of derangement at Cambridge was as follows:—

At 1 ^h 23' A. M., Gott. M. T., the reading of the scale was	111.9
“ 2 35' “ “ “ “ “	Range of 52'.4 East in 1 ^h 10'. 164.3
“ 3 35' “ “ “ “ “	Range of 52'.9 West in 1 hour. 111.4
Again, at 5 ^h 20' A. M., Gott. M. T., the reading of the scale was	108.5
“ 5 ^h 45' “ “ “ “ “	Range of 47'.6 East in 25 minutes. 156.1
“ 6 ^h 45' “ “ “ “ “	Range of 52'.4 West in 1 hour. 103.7

During the first of these periods, the aurora reached its culmination of splendour; between 5^h and 7^h it was faint and near the

* See also the *Journal of the Franklin Institute* for June, 1840, which contains some observations made upon it at Southwick, Mass.

horizon. It does not appear, from an examination of the May or August term-day, that the maximum agitation of the magnetometer coincides in time with the greatest brilliancy of the heavens. In May, it had not accumulated its action when the aurora began to decline; and in August, although it accompanied the display, it continued with undiminished energy one or two hours after that had passed away. The most rapid motion of the bar was from $5^h 20'$ to $5^h 45'$, being equal to $47'6''$ in 25 minutes. This is nothing strange; but might be expected from the time which all the forces of nature consume in communicating themselves to bodies, and penetrating large masses so as to overcome their inertia. More exact and frequent observations will doubtless conduct to a better knowledge of a connexion which is now so undeniable, and yet so imperfectly understood. If observers are careful to note the times at which the chief phases of the aurora are witnessed, and its position among the stars, and, where they have the opportunity, the simultaneous variations of the magnetometer, we may not despair of elucidating these two classes of intricate and interlaced facts—the aurora and the irregular perturbations of the magnetic meridian. It must not be inferred that other causes do not exist, in co-operation with the auroral phenomena, to derange violently the earth's magnetism. According to Ampere's theory of currents, a large fund of such derangements must be deposited under the crust of the earth. The equilibrium, although permanently stable, must be subject to constant fluctuations. Theory supplies the reason, and observation asserts the fact. Many of the small daily derangements have no apparent relation to the aurora; and in regard to the magnificent strides of the magnetometer, it cannot be told which is cause and which is effect. If further search shall prove that an aurora never fails to attend a great disturbance, we may conclude that the aurora itself is seldom displayed in the daytime; for the remarkable changes of declination almost always begin during the night, and seldom continue into the next day. If, however, an unseasonable aurora should occasionally arise, we may be able to perceive indications of its presence from the magnetic perturbations, although its light were eclipsed by the brightness of the sun.

So far we have attended to relative only, and not to absolute declinations. The former are sufficient when the object is to find the times of maxima and minima, the daily range, and the diurnal curve. But it sometimes becomes necessary to know the absolute declination, so that the process will now be described of referring any reading of the scale to its absolute value. It is clear that if the absolute value of one reading can be ascertained, that of all the rest is known at once. It is convenient to have the absolute declination always referred to the same number of the scale; we will suppose this number to be, therefore, 100. To find, then, the absolute declination corresponding to 100 of the scale, we proceed thus:—The variation-transit, with which the Gauss magnetometer is observed,

is placed firmly on the table, and then the line of collimation is adjusted in the meridian by means of polaris and the large transit-instrument; a section having been made in the roof for this purpose. The azimuthal circle is then read off. The circle is now turned until the line of collimation coincides with the direction of the magnetic meridian, as indicated by the needle that accompanies the *variation-transit*. At the same moment the circle is read off again, and the scale is noted through the telescope. The difference of readings from the azimuthal circle will indicate the angle which the magnet makes at that time with the astronomical meridian, and the reading from the scale shows to what number on it this absolute variation corresponds. Now since, according to their arrangement on the scale at Cambridge, an increase of numbers implies a decrease of declination, we readily find the absolute declination of 100 of the scale by adding or subtracting, as the case requires, the difference between 100 and the reading at the time. This will be easily understood from the following example:—

The azimuthal reading by Polaris, June 21, at 8 P.M.,

Gott. M.T. = $310^{\circ} 34' 40''$

The azimuthal reading at the coincidence of the

needle = $301^{\circ} 15' 30''$

When the reading of the scale through the telescope

was 100,835, the absolute declination = $9^{\circ} 19' 10''$

The absolute declination at 100 of the scale = $9^{\circ} 20' 00''$

As it may not always be possible to take an astronomical observation, on account of the state of the atmosphere, the azimuthal angle between some fixed mark and the true meridian is read off, and the position of the magnetic meridian determined by reference to this. Thus it appears, June 25th, that a certain mark on Gore Hall, which has been previously found to be $38^{\circ} 11'$ west of the north, reads on the circle—

	272° 23' 30"
Adding its azimuth or	38 11

The azimuthal reading of the true meridian	= $310^{\circ} 34' 30$
--	------------------------

The azimuthal reading at the coincidence of needle	= $301^{\circ} 14' 50$
---	------------------------

Absolute variation for 102.966 of the scale	= $9^{\circ} 19' 40$
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“ “ for 100	= 9 22 38
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The absolute variation corresponding to 100 of the scale being known, the real values of all the lower numbers are found from it by adding, and of all the higher numbers by subtracting, the difference between them and 100. Here we suppose, of course, that all the readings on the scale are made in the same position of the tel-

escope as the one by which the original absolute variation was determined. To secure this condition, when the observations begin, the azimuthal circle must be firmly clamped at some place which is considered the fixed reading for this period; and the vernier should be occasionally examined, to see that the instrument has not been deranged. The absolute declination thus obtained cannot be relied on within so small a limit of error as that to which the changes of declination are subject. The chief uncertainty attaches to the coincidence of the needle with the line of collimation. Several readings, repeated in succession, are likely to vary three or four minutes, so that their mean is only an approximation to the truth. Hence the difficulty of ascertaining the yearly change of declination, which is so small as to be partially masked under accidental errors. If the feet of the variation-transit were firmly secured to some durable foundation, the yearly variation might be found at once from the scale, as we now find the daily ones. In this case, the fixed reading should not be altered from one set of observations to another. Hereafter, as the observations will be made with Lloyd's declination magnetometer and a fixed telescope, we shall not be subjected to this inconvenience.

For greater accuracy, the absolute variation assigned to 100 of the scale for any period should not depend upon a single set of readings. But the process which has been described for finding the real value of any part of the scale, should be repeated as often as possible during the days of regular observations. Thus we have:—

June 21 at 8 ^h 0' P.M., Gott. M.T.,	absolute variation	= 9° 20' 00"	at 100 of the scale.
June 22 at 11 ^h 15' A.M.	" "	= 9° 21 14	" "
" 25 " 12 22 P.M.	" "	= 9° 22 34	" "
" 28 " 9 35 "	" "	= 9° 24 25	" "
" 29 " 3 26 "	" "	= 9° 21 44	" "
" " " 12 00 M.	" "	= 9° 20 53	" "
" 30 " 11 25 P.M.	" "	= 9° 18 29	" "
July 1 " 0 45 "	" "	= 9° 18 57	" "
" " " 11 25 "	" "	= 9° 20 36	" "
Mean for June		9° 20 59	" "
October 20 at 7 ^h 30' P.M.	" "	= 9° 18 15	" "
" 21 " 9 40 "	" "	= 9° 18 29	" "
" 22 " 10 5 "	" "	= 9° 16 40	" "
" 23 " 8 15 "	" "	= 9° 19 11	" "
" 24 " 2 10 "	" "	= 9° 16 58	" "
" 24 " 10 10 "	" "	= 9° 16 54	" "
" 25 " 2 45 "	" "	= 9° 15 56	" "
Mean for October		= 9° 17 29	" "

We now pass from the absolute value in declination of 100 of the scale to the absolute declination in this way. If the absolute variation for any single moment were desired, we should readily find it by adding or subtracting, as the case required, the difference between the reading on the scale and 100 to the absolute value of 100. But when speaking of the absolute variation, we generally intend some mean value which is the representative of that element for a whole day or month, or perhaps a longer period. We should certainly miss of this mean variation, if we adopted the regular maximum or minimum reading of the scale, or that of any extraordinary stride which may have been observed during the period. No single mark on the scale can lead to anything more than a momentary expression of this element. The mean of the daily maximum and minimum limits of the scale, or, more accurately, the mean of all the observations, furnishes a mark from which some durable value of absolute declination may be derived. Now, the mean of all the observations belonging to the 10 days of June is 103'.603 of the scale :

Hence the variation sought is..... $9^{\circ} 20' 59'' - 3' 36''$
or $9^{\circ} 17' 23''$.

Again, the mean of the observations made
during the 5 days of October is 98'.838 ;
hence the variation is $9^{\circ} 17' 29'' + 1' 10''$
or $9^{\circ} 18' 39''$.

The mean absolute declination from June
to November, 1840, may be considered = $9^{\circ} 18' 01''$.

The following table shews the variation of the needle at Cambridge and in the vicinity, from the period of the earliest observations :—

Cambridge 1708	9° 0' W.	Cambridge 1788	6° 38' W.
" 1742	8 0 "	Boston 1793	6 30 "
" 1757	7 20 "	Salem 1805	5 57 "
" 1761	7 14 "	" 1808	5 20 "
" 1763	7 0 "	" 1810	6 22 "
" 1780	7 2 "	Cambridge 1810	7 30 "
Beverly 1781	7 2 "	" 1835	8 51 "
Cambridge 1782	6 46 "	" 1840	9 18 "
" 1783	6 52 "		

As there can be no great difference between the variation for Boston and Cambridge,* we infer, from the table, that from 1708 to 1793 the declination diminished at the mean annual rate of 1'.8,

* The following values of the latitude and longitude are taken from the American Almanac for 1840 :—

	Latitude.	Longitude.
Boston	42° 21'	71° 4'
Cambridge	42 22	71 8
Salem	42 31	70 53

and that from 1810 to 1840 it increased at the mean annual rate of 3'.6. It is probable that the change in direction took place between 1793 and 1810 at Cambridge; whatever was the cause, it does not appear to have affected instantaneously remote places. Dr. Bowditch, who made the observations at Salem in 1805, 1808, and 1810, supposed that the first two were smaller than they ought to be, on account of instrumental errors; but I think that this was not the whole cause. The change in direction, if it had not already happened in 1810, came soon after. Now, on the assumption that it was after 1810, the mean annual rate of decrease at Salem (considering the observations at Beverly as comparable with those made at Salem) would be only 1'.3. This is smaller than the decrease appears to have been on the average. I incline to think, therefore, that the minimum declination really occurred during the period of Dr. Bowditch's observations, and that the differences which he attributed wholly to defects in the instruments, were partly caused by this very circumstance. The value of the variation at Beverly in 1781 was obtained from the mean of 7 partial means, which did not differ more than 6 minutes from each other. The variation at Salem in 1810 was the mean of 5125 observations. Confidence may accordingly be placed in both of these values, and consequently in the mean annual decrease of 1'.3 that results from them. Again, the variation at Boston in 1793 is the mean of 1644 observations. The variation at Cambridge in 1782 is deduced from frequent observations at different hours on 127 days of that year. Allowing a reasonable error to beset the value at 1708, we may suppose that the number of years from which the rate of annual decrease is derived secures it from any large error. The same reliance may reasonably be placed in the determination of the rate of increase. The fact, then, is very remarkable, that since the passage from a direct to a retrograde motion the rate of the annual change of variation has so materially altered. Assuming that the mean annual rate of decrease was 1'.8,* the time of the change interpolated into the observations would be 1807; and this, in the absence of better authority, may be regarded as its date for Cambridge.

Another element of the earth's magnetism, on which some attention has been bestowed at Cambridge, is the dip. All the methods of observing the dip are extremely defective, and do not admit of so great a degree of accuracy as the declination instruments. The dipping-needles of Gambey, and Troughton and Simms, are preferable to any other direct method of measuring absolute dip. But two of the best dipping-needles may vary 15' and more, in the determination of this element for the same time. This difficulty

* In most cases of this kind, the rate varies about the times of maximum and minimum as the time, reckoned from these points respectively; so that the value of the element is in proportion to the square of this time. But in the present instance the simple supposition we have made in the text conforms best to the observations.

suggested to Gauss the idea of expressing the dip as a function of the horizontal and vertical components of the magnetic intensity. This led to the invention of his bifilar magnetometer, and the horizontal force magnetometer of Lloyd. Professor Lloyd has added to these another extremely delicate instrument, by which *variations* of the vertical force may be observed. Consequently, with his two instruments, the *changes* of dip may be calculated to a close approximation, though the absolute dip remains unaffected by all the improvements. In addition to the usual corrections applied to observations made with the dipping-needle, the precaution should be taken of observing the inclination in different azimuths, and deducing the true dip from every set at right angles to each other. Mr. Fox has invented a dipping-needle deflector, which gives the dip and intensity by a statical principal, and Mr. Lloyd has applied the same principle to the simultaneous determination of the dip and intensity. Observations with the vertical force magnetometer commenced with us on the term-day of March, 1841; but they do not enter into the plan of the present paper, and any further notice of them, or of the instruments, is deferred till we come to the description of the new magnetic observatory. The dip has been observed directly at Cambridge and the vicinity, during the last four years, at irregular intervals. The dipping-needles used were of the best construction, one of them made by Gambey, and the other, now in the possession of Major Graham, obtained from Troughton and Simms. The following table gives the results, with some old observations on the same element:—

Place.	Date.	Dip.			Gambey two needles.	Troughton & Simms one needle	Means.
Cambridge	1780	69°51	Dorchester	June 8	74°20.3	74°25.6	74°23'
"	1782	69 41	Cambridge*	" 10	74 17.1		71 17.1
"	1783	69 41	"	"	74 20.1		74 20.1
			"	June 22	74 24.7		74 24.7
Mean for	1782	69 44*	"	"	74 14.1		74 14.1
			"	Sept. 6	74 22.5	74 11.5	74 17
Cambridge	Lat. 42°22	Long. 71° 8	"	1841			
Dorchester	42 19	71 4	"	May 9	74 35.5		74 35.1
Mean for				1840			74 21.6

Supposing that the inclination has been on the increase since 1782, its mean annual rate would be equal to 4'.5. It is thought that the inclination is decreasing at present in the United States, as it has long been in Great Britain and on the continent. Professor Loomis assigns the rate for this country at 1'.8 yearly. If this be so, we

* This observation was made by Professor Loomis.

observe the same great difference in the rate of increase and decrease of the inclination as of the declination. In either case, it is equally inexplicable. The want is now felt of systematic observations of the dip, in order to eliminate regular and irregular changes of this element, and obtain a true mean for a given period. The general scheme of magnetic observations has not failed to provide for this, as we shall see further on. So far as the subject has been investigated, the dip does not appear to pass through such a uniform succession of positions daily as the declination. Its value often changes suddenly, and to the amount of a degree and a half, although, according to Kupffer, the regular daily range does not exceed five minutes. As the various observations on the dip, at Cambridge and Dorchester, were made with the same needles, the range of 21' in the values cannot be charged to the needles, and it is too great to come under the *daily* or *annual* variation. Observations made elsewhere shew that such irregular disturbances occasionally derange the dip. Moreover, the mean value, derived from frequent partial results obtained on different days and different hours of the same day, is still exposed to what may be called a constant error of the needle, and which is not eluded by any of the different reversals. Thus, out of 8 needles used by Captain James Ross, in London, 2 differed 41' in the values which they gave for the dip; although from 640 to 1,000 readings were made with each. We subjoin the following table, drawn up by Quetelet,* which contains the value of the annual diminution of the magnetic inclination at various places:—

Paris	3. '7	Stockholm	3. '13
Brussels	3. 4	London	2. 4
Berlin	3. 7	Dublin	2. 3
Turin	3. 5	Christiana	3. 56
Florence	3. 3	Gottingen	3. 05
Milan	3. 87	St. Petersburg	3. 8
Upsal	3. 27		

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* Nouveaux Mémoires de l'Académie Royale des Sciences et Belles Lettres de Bruxelles. Tome xii. 1839.

(To be completed in the next Number.)

MAGNETIC PHENOMENA.

[For Declination read Variation.]

On the 25th of September of the present year, a most extraordinary disturbance of the magnetic instruments was observed at the Magnetic Observatory attached to the Royal Observatory of Greenwich. The disturbed state of all the instruments attracted the attention of Mr. Glaisher (chief assistant in the magnetic department), at an early

hour of that day, and he immediately commenced a series of observations on all the instruments, at short intervals. After a time, the disturbance became so small that the extraordinary observations were discontinued; but it again increased, and observations were again immediately made with all the instruments, and repeated as rapidly as it was possible for one observer to do so. But for the promptitude and judgment displayed by Mr. Glaisher on this occasion, the record of this disturbance, unprecedented in this magnetic latitude, would probably have been lost. The following statement will give an idea of the magnitude of the disturbance:—That within eight minutes of time the declination needle changed its position *more than* $2\frac{1}{2}^{\circ}$ (having passed in both directions the range of the observing telescope, which includes that angle); that the vertical force was increased by *more than* 1-40th of its whole value, the instrument having then reached the extremity of its range; and that the horizontal force was increased about 1-30th of its whole value.

The following more detailed account is taken principally from the abstract furnished by Mr. Glaisher. It is to be remarked, that the time throughout is Gottingen mean solar time, civil reckoning; that the readings of the theodolite for the declination needle increase as the north end of the needle moves towards the east, and that the reading for the astronomical meridian is $269^{\circ} 51' 45''$ nearly; that the increasing readings of the vertical force magnetometer imply increasing vertical force, one division being equal to 0.000471 of the whole vertical force; and that the increasing readings of the horizontal force magnetometer imply increasing horizontal force, one division being equal to 0.002214 of the whole horizontal force.

Early in the morning of Sept. 25th, the needles were in an agitated state. During the appearance of an aurora, additional observations were taken, and the declination needle in less than three hours traversed an arc of $34'$. After this, the needles were in a tolerably quiet state, and extraordinary observations were discontinued.

The observations at 10^h A.M., showing a change of $17'$ of arc from the previous reading, at 8^h A.M. extra observations were again resumed, and continued till 11^h A.M.; nothing remarkable appearing during this time, they were again discontinued.

At 2^h P.M., Gottingen mean time, it was evident that all the needles were affected by some unusual change of disturbance, and incessant observations were at once commenced, the instruments being observed successively, as quickly as one observer could take them. At 3^h 53^m Mr. Hind joined, and subsequently Messrs. Dunkin and Paul. From this time to the discontinuance of the observations two persons were constantly engaged, one taking the observations with the vertical force magnetometer, the other those of the declination needle and of the horizontal force magnetometer. About 2^h 40^m P.M., the motion of the needles became very peculiar; the vibrations of the declination needle, and of the vertical force magnetometer, being quite destroyed; and those of the horizontal

force magnetometer being reduced to a continuous succession of jerks or starts. It will perhaps contribute to clearness if the motions of the three instruments are now separately described.

Declination Needle.

Every change in this instrument from one position to another was by sudden impulses; after each of those it was stationary, for intervals varying from 5' to 20'; then it was forced again to another position, and was again stationary without the slightest motion; and so on successively.

At 3^h 30^m the changes from one position to another became very decided, but the motion still partook of the same character; absolute rest and jerking motion followed alternately; in 2' or 3' the needle frequently moved through many minutes of arc, and then suddenly became stationary again.

At 3^h 36^m 20^s P. M., a bold sweep carried the north end of the needle towards the west 56' of arc, in one minute of time; at 3^h 36^m 20^s the theodolite read 245° 22'—; and for two minutes after this time the cross carried by the needle was out of range of the telescope,* being in a position in which, if the telescope had been exactly directed to it, the theodolite would have read less than 245° 22'. At 3^h 40^m 20^s P. M., Gottingen mean time, the needle had moved back 25'; at 3^h 43^m 50^s the theodolite read 246° 19'; at 3^h 44^m 20^s the needle had moved so far (north end towards the east) that the theodolite reading was 247° 38'+, and the cross carried by the needle was out of range of the telescope on the opposite side.

This extraordinary magnetic stride of more than 2½° was traversed in about 8", the extremes being only separated by that interval of time.

At both extremities the needle was without swing, all momentum from such a wide sweep being quite destroyed. After remaining at this place for more than two minutes, the needle slowly returned, and at 3^h 48^m 54^s the theodolite read 246° 27'; at 3^h 53^m 21^s it read 246° 37'. Within 5' after this, another bold sweep carried the cross out of the range of the telescope; when found, at 3^h 54^m 40^s, the theodolite read 245° 31'+; instantaneously it rushed back, with a violently agitated motion, across and out of the field on the other side; the circle-reading, at 3^h 55^m 0^s, was 246° 43'+, and therefore this arc of 1° 12'+ was passed over in 20' of time.

* This arose from the great inclination of the axis of the collimator (carried by the magnet) to the magnetic meridian; in consequence of which the pencil of rays passing through the object-glass of the collimator was thrown entirely on one side of the object-glass of the theodolite. In ordinary cases, the change of angle has been sufficiently slow to allow the observer to shift the suspension of the magnet; but the rapid changes on September the 25th did not allow the observer to leave his place. Observations with the mirror in Gauss's manner are free from this inconvenience.

This was the last very great excursion; the needle was incessantly watched for 9 $\frac{1}{2}$ longer, during which time it was much agitated. The most remarkable changes are the following:—

h. m. s.					
At 4	25	36	P. M., Gottingen Mean Time,	Theodolite read	246° 8'
	31	36	"	"	34
	49	36	"	"	8
4	55	36	"	"	21
5	46	37	"	"	10
	50	37	"	"	30
6	0	30	"	"	8
6	33	37	"	"	26
6	46	36	"	"	32
7	16	38	"	"	25
7	31	38	"	"	46
7	56	38	"	"	37
8	53	38	"	"	28
9	18	38	"	"	43
9	38	38	"	"	51
10	10	39	"	"	53
10	41	39	"	"	39
10	46	39	"	"	46
11	45	40	"	"	30
12	52	40	"	"	47

It is to be remembered, that increasing readings denote that the north end of the needle moves towards the east.

Vertical Force Magnetometer.

The vertical force magnetometer was as much affected as the declination instrument. The disturbances of its vibrations were very similar, except that it was frequently much longer in the stationary state, its times of rest varying from 5^s to 2^m; at the end of each of these it jumped on, then was again still, and so on.

This instrument had been much affected early in the morning, the extent of its excursions being between 37^d.9, and 44^d.1 on the scale. The following are scale-readings, commencing with noon:—

h. m. s.					d.
At 0	0	0	(noon), Gottingen Mean Time,	scale-reading	44.0
	1	47	30 P.M.	"	47.8
	1	57	30	"	48.1
	2	7	30	"	50.0
	3	15	21	"	65.0
	3	16	21	"	55.9
	3	21	26	"	57.9

At 3 22 21	P. M., Gottingen	Mean Time, scale-reading	58.3
3 22 51	"	"	59.0
3 23 21	"	"	60.3
3 23 51	"	"	60.3
3 24 21	"	"	60.4
3 26 21	"	"	63.0
3 27 51	"	"	63.2
3 31 21	"	"	67.0
3 32 21	"	"	69.5
3 32 51	"	"	71.2
3 34 21	"	"	81.3
3 34 51	"	"	95.0

3 35 21, the marked end of the needle (which is towards the east, the needle being transverse to the magnetic meridian) was resting on the eastern Y, or apparatus intended for guarding the needle, dipping as much as the frame permitted it, and held there apparently by a great power; in this situation it remained for ten minutes, till 3^h 45^m.

h. m. s.		d.	
At 3 45 42	P. M., Gottingen	Mean Time, scale read	73.1
3 46 23	"	"	68.9
3 50 31	"	"	63.4
3 57 38	"	"	57.7
3 58 58	"	"	55.6
4 13 36	"	"	62.9
4 29 26	"	"	66.9
5 21 37	"	"	55.6
5 44 37	"	"	60.6
5 55 37	"	"	53.2
6 19 7	"	"	68.3
7 17 8	"	"	53.8
7 35 8	"	"	50.3
9 22 9	"	"	45.6
Sep. 25, 11 0 39	"	"	40.9
Sep. 26, 0 58 25	A. M.	"	33.7
1 3 25	"	"	36.1

About 6^h 19^m P. M., the needle assumed a steadier appearance, having a motion something like vibration, but exceedingly irregular.

Horizontal Force Magnetometer.

The horizontal force magnetometer was not less affected than the other two instruments. The effect on its vibrations was, however,

peculiar to itself: in the midst of its swings it was suddenly checked, and held in that place for a second or two, then hurried backwards, again suddenly stopped, and then forced back in the contrary direction. These checks were sometimes at 5° , sometimes at 10° , after arriving at its limit. They were most striking. From experience with this instrument, we are accustomed to look for an almost imperceptible motion at the extremities of the vibrations, then to see it move slowly in the opposite direction, quickly in the centre of swing, slowly towards the end, then return, and so; but to see it, almost in the same instant, moving rapidly and absolutely motionless, was very striking and strange to the observers.

This instrument was less affected in the early part of the day than either of the others. About noon it showed signs of being under the influence of some disturbing cause: the scale then read $55^{\text{d}}.8$; at $1^{\text{h}} 50^{\text{m}}$ the reading increased to $58^{\text{d}}.6$; at $2^{\text{h}} 0^{\text{m}}$ to $59^{\text{d}}.3$; at $2^{\text{h}} 10^{\text{m}}$ to $60^{\text{d}}.1$; it then oscillated between 59^{d} and 61^{d} , till $3^{\text{h}} 11^{\text{m}}$, the scale then reading $59^{\text{d}}.7$. The disturbances now became as surprising as those with the other needles; a steady and rapid increase of scale-reading took place, with a strange jerking motion.

	h.	m.	s.		d.
At 3 14 48				P.M., Gottingen Mean Time, the scale read	61·4
3 21 3				" "	61·2
3 27 26				" "	64·2
3 28 6				" "	64·4
3 28 46				" "	64·8
3 38 16				" "	73·6
3 38 56				" "	73·2

Then there was a remarkable start to $78^{\text{d}}.0$; and here it remained stationary from $3^{\text{h}} 39^{\text{m}}$ to $3^{\text{h}} 43^{\text{m}}$, when it started back.

h. m. s.	d.
At 3 47 41 P. M., Gottingen Mean Time, the scale read	69·9
3 56 31	65·6
4 0 51	63·7
4 8 55	63·1
4 15 11	64·6
4 47 41	61·4

Then the reading increased a little, and then gradually decreased, till 5^h 53^m 12^s P.M., Gottingen mean time, when it read 57^d.8; it then increased to 6^h 16^m 12^s, when the scale read 62^d.1.

It then oscillated between 58^{d} and 60^{d} , until $7^{\text{h}} 25^{\text{m}} 13^{\text{s}}$, when a

sharp diminution to $53^{\circ}.9$ took place. The readings alternately increased and decreased for the next six hours, oscillating all the time between 53° and 57° .

From the preceding account it will be seen that the magnetometers were more affected on this day than ever before, and that the great disturbance affected the three instruments at the same time.

The most abrupt and violent fluctuations of the meridian needle occurred between $3^{\text{h}} 36^{\text{m}}$, and $3^{\text{h}} 46^{\text{m}}$. At $3^{\text{h}} 35^{\text{m}} 21^{\text{s}}$ the vertical force magnet went beyond the scale: at $3^{\text{h}} 45^{\text{m}}$ it again came on. The horizontal force needle began its great deflection before $3^{\text{h}} 38^{\text{m}} 16^{\text{s}}$; it was over at $3^{\text{h}} 48^{\text{m}}$; and, probably, had there been an observer at each instrument, it would have been found that the effects on all were simultaneous. Such was Mr. Glaisher's impression while observing; but, as he remarks, the startling and bewildering effect of so great changes was such, that all that he could do was to make such observations as seemed practicable, with a view of discovering the *extent* of such extraordinary disturbances.

After this great disturbance the irregularities were still very great; the horizontal force and vertical force magnetometers partook of similar disturbances throughout. With a few trifling exceptions, an increase of force in one was accompanied with an increase of force in the other, and so also with respect to the decrease of force.

The declination magnet has certainly, however, one great deflexion, in which the others have no share; that which occurred between $3^{\text{h}} 54^{\text{m}} 40^{\text{s}}$ and $3^{\text{h}} 55^{\text{m}} 0^{\text{s}}$, where an arc of $1^{\circ} 12'$ was described in 20. Nothing similar, at this time, is shewn by either of the other needles.

In addition to the observations contained in this account, ample and abundant observations were taken to obtain every variation of the vertical force, and also observations of the declination and horizontal force magnetometer, sufficient to shew every great change, and nearly all the small ones, until Sep. 26^d 1^h A.M., when the observations were discontinued.

At $4^{\text{h}} 40^{\text{m}}$ an observation of the dip was taken, giving a result greater than ordinary.

The day (Sept. 25th) was cloudy throughout: about 9^h P.M. a few bright streamers were seen through the clouds; then nothing more till 11^h P.M., when an auroral arch, about 24° high, was visible for a short time.

It is desirable that accounts of corresponding observations of this disturbance should be collected. I invite observers, who may have made observations on the same day, to send to me abstracts of their observations, or to communicate them in the way which they think best to the public, or to the persons who are interested in magnetical phenomena.

G. B. AIRY.

Royal Observatory, Greenwich, 26th Oct., 1841.

THE VARIATION OF THE COMPASS.

Royal Observatory, Dec. 22nd, 1841,
Magnetical and Meteorological Department.

MEAN MAGNETIC VARIATION FOR OCTOBER—23° 12' 18"

MEAN MAGNETIC DIP FOR OCTOBER.

At 9 A.M.

—68° 43'

At 3 P.M.

68° 35'

MEAN MAGNETIC VARIATION FOR NOVEMBER—23° 17' 7"

MEAN MAGNETIC DIP FOR NOVEMBER.

At 9 A.M.

68° 25'

At 3 P.M.

68° 37'

G. B. AIRY, *Astronomer-Royal.*

A Register of the Electric State of the Atmosphere, in relation to its Meteorological Phenomena, for the month of August, 1841, at Sandwich. By W. H. WEEKES, Esq., Surgeon, Lecturer on Philosophical and Operative Chemistry, &c.

AUGUST, 1841.

Day of Month.	Time, in hours and minutes.	Thermometer.	Barometer in inches and hundredths.	Wind, its direction and character.	Weather and modifications of clouds.	Electrical condition of atmosphere	General Remarks.
1		63	29.50	brisk N.W.	Blue sky, with light striated and feathery cirri.	positive	Generally free, and at times strong divergence.
2		66	29.58	brisk W. N.W.	Fleecy cumulus in rapid transit. An occasional nimbus.	positive	Frequent strong divergence.
	6 p.m.			decreased to a gentle breeze.	Slight shower. Continued rain through the night.	neutral	Many cases of catarrh, and some with symptoms of influenza. Pertussis continues prevalent.
3		66	29.33	light breezes S. W. by W.	Dense veil of cirro-stratus cloud.	neutral	Sensation of extreme languor and oppression very general. Odontalgic affections are rather prevalent.
	4 p.m.			breeze increased.	Manifold cumulo-floccosus.	positive	Very slight divergence. Abatement of languor, &c.
	7 p.m.		29.13	increasing N. W. by W. stormy through the night.	Continued heavy rain.	neutral	
4		58	29.12	brisk gale W. N. W.	Nimbus and cumulo-floccosus.	neutral	
	2 p.m.	66	29.23	strong breeze N. by W.	Blue sky, with an occasional modification of cumulus.		Slight divergence, almost constant.

Day of Month.	Time, in hours and minutes.	Thermometer. Barometer in inches and hundredths.	Wind, its direction and character.	Weather and modifications of clouds.	Electrical condition of atmosphere	General Remarks.
5	3, 15 p.m.	66 29.42	gale S. W. slightly abated.	Blue sky, with a variety of cirrus clouds. Frequent showers of fine driving rain.	positive neutral	Strong steady divergence until one P. M.
6	2 p.m.	65 29.42	brisk W. S. W. increasing	Cumulo floccosus obscuring the sun. Clouds divide, and large flocculent masses arrive in succession. Fine driving rain.	neutral positive neutral	Frequent moderate divergence until approach of sunset.
7	night a. m.					Rising barometer.
	2 p.m.	69 29.57	breeze, W. S. W.	Manifold fleecy cumulus clouds	positive	Moderate divergence, increased when solar rays penetrate the clouds.
	9, 35 p.m.		stronger S. W. smart breeze, S. W.	Blue sky, with light fleecy cirri. Transparent blue sky from early eve. — Moon is rising	positive positive	Increased divergence.
8	3, 30 p.m.	67 29.40	light breezes, W. S. W. slight, S. W.	Clouds interspersed with haze. Sun invisible. Fine rain.	neutral neutral	Divergence ceases, having declined since sunset.
9	12, 45 p.m.	63 29.30	none	Cloud and thick haze.	neutral	
	3, 40 p.m.			Haze dispersing. — Light cirrus clouds.	positive	Immediate divergence, continuing only a few seconds.
	p.m. night			Nimbus cloud and hasty shower. Clear blue sky. Serene clear sky, with moon in last quarter.	positive positive neutral	Free discharge of sparks.
10	morning.		breeze, W.	Blue sky continues.	positive	Free divergence until sunset.
	8, 40 a.m.			An immense nimbus has suddenly arrived. Hasty showers.	negative	Annual "meteoric shower," observed from eleven until one.
	p.m.	64 29.04	breeze, S. W. stormy, S. W.	Manifold cirri.	positive	Tremendous torrent of sparks, effecting rapid decomposition of fluids.
	night			Heavy rains.	variable	Occasional divergence.
11	a.m.	62 29.25	stormy, S. W.	Driving distended sheet of cloud.	neutral	Frequent powerful currents.
	2 p.m.	66 29.15	gale, S. W. abated W.	Blue sky and fleecy cirri.	positive	Slight divergence.
	evening.			Gentle showers.	neutral	Obstinate diarrhoea is somewhat prevalent.
12		64 29.50	breeze, W.	Blue sky, with occasional cirrus clouds.	positive	Strong divergence.

Day of Month.	Time, in hours and minutes.	Thermometer.	Barometer in inches and hundredths.	Wind, its direction and character.	Weather and modifications of clouds.	Electrical condition of atmosphere	General Remarks.
13	6, 20 p.m. night	63	29-50	breeze, W. S. W.	Modifications of cumulus, with cirri in a higher region.	positive	Free divergence almost uniformly.
				none	Light nimbus clouds without rain.	negative	Moderate divergence.
				breeze, S. by W.	Several showers.	neutral	
14		65	29-38	brisk and variable from S. W. to W.	Various modifications, with cirri in a higher region.	positive	Occasional moderate divergence.
15	morning.			breeze, S. W.	Several showers of very fine rain.	neutral	
		65	29-40		Various modifications, with feathery cirri in a higher region.	positive	Moderate divergence generally.
16		68	29-50	breeze, W. by N.	Light cirri and blue sky. — Cumulus clouds occasionally.	positive	Free divergence generally.
17		68	29-60	breeze, S. W.	Cumulo floccosus manifold. — P. M. less cloudy.	positive	Occasional moderate divergence.
18	evening.	69	29-70	decreased light breeze, variable zephyrs, S. W.	Hazy upward. Thickly clouded.	neutral positive	Trivial divergence at times, when sunbeams momentarily penetrate.
19	4 p.m.	68	29-75	light breezes, S. by E.	Clear blue sky.	positive	Steady moderate divergence.
20		68	29-52	faint breezes, variable from S. by E. to E. N. E.	Blue sky without a cloud.	positive	Steady moderate divergence from sunrise to sunset.
21		68	29-39	brisk S. W.	Transparent blue sky.	positive	Continuous current of minute sparks from sunrise to sunset; moderate divergence continued until wind ceased about 9 P. M.
22		66	29-52	brisk S. W.	Clouds with intermediate haze, penetrated by sun's rays, at times.	negative	Moderate divergence when sun's rays penetrate.
	11 a.m.				Nimbus clouds, with an occasional hasty shower of very small rain.	negative	Currents of minute sparks.
					Rocky cumulus; various cirri in a higher region. — P. M. Clear blue sky.	positive	Moderate divergence generally, increased at times.
23		65	29-51	light breezes, variable from S. W. to W. N. W.	Very light showers occasionally.	negative	Moderate divergence during showers, neutral in the intervals.

Day of Month.	Time, in hours and minutes.	Thermometer.	Barometer in inches and hundredths.	Wind, its direction and character.	Weather and modifications of clouds.	Electrical condition of atmosphere	General Remarks.
24	11, 20 a.m.			light breezes, S.W. slight breeze, W.N.W.	Clear blue sky.	positive	Constant free divergence.
	2 p.m.	64	29.66	breeze, variable from W.N.W. to N.	Picturesque skies from cirro-cumulus and distant cumulo-stratus. Blue sky, with fleecy and rocky cumulus at times.	neutral positive	Free divergence, with short intervals of suspension.
25		63	29.66	very light breezes, from S.W.	Occasional fine rain.	neutral	
26		69	29.79	light breezes, S.W.	Manifold cumulo floccossus in a blue sky.	positive	Slight occasional divergence.
27		70	29.82	faint zephyrs from S.W.	Blue sky with cumulo floccossus and various modifications of cirrus cloud.	neutral	General sensation of sultry oppressive heat, extreme lassitude, head-ache and slight nausea. No trace of free electricity can be detected.
	3 p.m.				Fewer clouds.	positive	Trivial degree of divergence until 6 P.M.
28		70	29.77	light breeze, N.E. by E. vacillating between N.N.E. and S. by W.	Blue sky, with fibrose cirro.	positive	Continuous free electric current; pendulums strike frequently.
	2, 35 p.m.				A limited thin veil of cirrus, passing beneath manifold fibrose cirro.	positive	A remarkably strong and well defined solar halo lasting 18 minutes. Free divergence until approach of sunset.
29		73	29.67	light breeze, E.N.E. breeze gradually dying away.	Azure sky.	positive	Strong divergence, with striking of pendulums.
	3 p.m.					positive	Divergence fast decreasing.
30		74	29.58	light breeze, S.W.	Azure sky.	positive	Continuous moderate divergence.
31		75	29.44	light breezes, W.	Azure sky.	positive	Moderate divergence.
	3, 20 p.m.			faint zephyrs, N.W.	Cirro-cumulus extending over visible hemisphere.	negative	Slight divergence.
	4 p.m.				Occasional rain-drops.	positive	Increased divergence.
	4, 18 p.m.					neutral	Subsequently ascertained the occurrence of a thunder-storm at this time, distant about eight miles to the N.N.W.

The continuation of electroscopic divergence until a late hour after sunset, on the evening of the 7th, with a transparent blue sky, and the wind at S.W., is an unusual circumstance, and might possibly have some relation to the neutral state of the atmosphere which prevailed during the next thirty-nine hours, and to the phenomena immediately succeeding on the afternoon of the 9th.

The commencement, as I presume, of the annual August "meteoric shower," about eleven o'clock on the night of the 9th, was seen here, by myself and a friend, to great advantage. The first observed flight of these brilliant bodies (some twenty in number) occurred at an altitude nearly midway between the horizon and the constellation *Ursa Minor*, apparently leaving luminous trains parallel to each other, and to the horizontal plane, for the space of a second or two; the latter appearance might probably be merely a delusive effect, produced by a sudden impression upon the retina. Within the space of thirteen minutes, subsequent to the first flight, seventeen other meteors were distinctly counted by myself, in addition to four seen by my companion in a southerly direction. The meteoric bodies observed to the northward seemed, as it were, to start suddenly out from the sky, by one, two, and three identically, passing, like the first shower, from E. N. E. to W. S. W. parallel to the plane of the horizon. A transparent blue sky had predominated from an early hour in the evening. The moon, just entering her last quarter, was rising at the moment these beautiful phenomena first became apparent; yet, between the hours of eleven and one, certainly more than one hundred meteors might have been successively enumerated, proceeding from a common centre, constituting a space in the heavens extending over about thirty degrees of a circle to the right hand, and considerably below *Ursa Minor*. As the moon gained in altitude, the reappearance of these splendid bodies became proportionately lessened; only those of the greatest magnitude probably escaping the influence of her increasing light. I had hoped to renew my observations on the meteoric shower during the night of the 10th; but we experienced here a densely clouded atmosphere, with a storm from the southwest, accompanied by drenching rains and powerful electric currents.

I think it will be sufficiently obvious, on reference to the preceding register, that the thunder-storm on the afternoon of the 31st, although it occurred about eight miles to the N.N.W., must have exercised an influence over the vicinal atmosphere of our exploring wire, thereby inducing a *neutral* condition in its immediate locality, at the identical time the distant storm was raging.

The general state of the atmosphere, during the month of August, would appear to have exercised a highly salubrious influence over the animal system; whence has resulted a freedom from disease beyond the average standard, not only in this district, but throughout the eastern parts of England. The only remarkable

departure from the above condition in my immediate vicinity was incident to about ten days after the 14th instant, when I noticed the frequent occurrence of somewhat obstinate aphthous affections, with swelling of the parotid and submaxillary glands in young persons, especially in females.

Sandwich, Sept. 1841.

Electrical Phenomena, in a Letter from JAMES MARSH, Esq., to WILLIAM STURGEON, Esq., Royal Victoria Gallery, Manchester.

Royal Arsenal, Woolwich, Dec. 28th, 1841.

My Dear Sir,—If you think the following worthy of a place in your excellent *Annals of Electricity*, &c. &c. &c., you will greatly oblige me by giving it a corner.

I am, my dear Sir, yours truly,
JAMES MARSH.

Some years since I happened to be in the shop of Mr. John Fry, grocer, &c., in High-street, Woolwich, and while there I had my attention drawn by his shopman to what appeared to him a very extraordinary phenomenon. He was engaged grinding of newly roasted coffee, and had observed, on former occasions, that when the receiving tin can, placed under the spout of the iron coffee mill, was at a short distance (say $\frac{1}{16}$ of an inch, or less in some cases) from the edge of the spout, a stream of electrical sparks were observed to fly from the edge of the iron spout of the mill to the edge of the receiving tin, which was placed on a block of wood close to the wood-post to which the coffee mill was attached. I made several experiments with an electrometer, dry tin cans, &c. &c., and under all circumstances was enabled to obtain a large quantity of sparks, but more in particular when the atmosphere was in a dry state, and everything about the mill dried and dusted.

We know that in the case of chocolate and many other substances, in cooling, and others by being heated, such as the electrical tourmaline, the calamine, &c., that signs of electricity are perfectly evident by their attracting light bodies towards them; but I do not recollect ever having seen the developement of strong sparks in succession, by the means herein described.

If any of your correspondents have observed a similar phenomenon, or if it has fallen within the range of your extensive acquaintance with the developement of this extraordinary agent, under the circumstances before stated, you will greatly oblige me by mentioning it in the next number of the *Annals*, or at your first leisure.

Some friends of mine have been amusing themselves by firing at penny pieces thrown up into the air at a few yards from the muzzle of the gun: in some cases rifle shot was used, but in other cases mostly small bird shot was employed. I send you a specimen of the effect of each, as I think the coins perforated by the bird shot

possess something about them worthy of your attention. The strong resemblance that the burr on both sides of the coin have for the burr produced by the electrical discharge through paper or card, has induced me to draw your attention to the fact. If you think it bears any resemblance in effect, you will greatly oblige me by giving an opinion on the subject.*

It appears to me the resistance that the bird shot meet with at the first moment of impact against the copper coin, causes a sudden splash or disruption of the surface first struck, producing the burr on that surface, and that the shot still continue on with the greater part of the original momentum given by the powder in the first instance, until they pass through the copper coin, producing the burr on the opposite side, after the manner of a punch when driven through metals by any ordinary force. Water also, when ejected with force on the ground, produces similar effects as respects the first surface acted on.

* The specimens with which Mr. Marsh has favoured us, are very striking instances of burrs being produced on *both sides* of bodies when perforated in *one direction* only; and shew the futility of all attempts to prove the existence of *two electric fluids* from the electric burr experiment.—EDIT.

Report on the New Processes introduced in the Art of Gilding.

By M. M. ELKINGTON and DE RUOLZ.†

A new art of the highest importance, for its tendency is, generally, to render the enjoyments of luxury more reasonable, has just had birth given to it in France, at least it has there received unexpected developements. It is the art of applying at will the metals which offer the greatest resistance, or the most beautiful, in layers as thin as those of varnish, or in thick layers, as may be required, on the objects formed of other metals less dear and more durable than they are.

Thus objects in iron, in steel, that is to say tenacious, hard, or sharp, but oxydable in the air, may, whilst they at the same time preserve their original properties, become unalterable by means of a varnish of gold, of platina, or silver, so light and so thin that their price will scarcely be increased in the least.

Utensils of copper, tin, or brass, which would be dangerous or disagreeable, may receive the same preparation in thicker beds or layers, and thereby become unalterable to the action of the air, inodorous and salubrious in their employment. And as the agent which produces these effects possesses a power without limits, it may be added, that it is not only gold, platinum, and silver which we may apply on some metals, but copper, lead, zinc, nickel, cobalt, &c., which, put under contribution according as the circumstances may require, come in their turn to change the aspect of the objects

† Comptes Rendu, December, 1841.

on which we force them to deposit themselves, or to communicate properties useful and novel.

It is enough to say, that the agent which determines these metallic precipitations is no other thing than the pile; but the pile applied to the dissolutions of a suitable nature, and of which, until now, the necessity has not been comprehended for these kinds of reactions.

We ask the Academy, with permission, to dwell some moments on an art which will have for its effects an almost certainty to destroy all those trades so dangerous as the gilding with mercury; which will transport, even into the most humble chamber, the agreeable and salubrious usage of plated utensils; which will permit the application of silver gilt to a crowd of objects of common usage, and which, by that, even provoking a considerable deperdition of the precious metals, will reanimate the exploration of the mines of silver—will enhance the abated value of this metal, and make an equilibrium in the excess of production, which in this sense has manifested itself for a long time in so striking a manner.

The Commission formed by the Minister of Finances, M. Lacave-Laplagne for the examination of our moneys, of our monetary trades, and the general melting down again of all our metals in circulation, will then see with pleasure a discovery which tends to correct an inconvenience with which it is itself actively pre-occupied, the excessive accumulation of silver in France, which in less than fifteen years has seen its capital of silver doubled, and at least 5-7ths of its capital of gold disappear. But she will see, perhaps, also, with some disquietude, so many causes which menace the situation of our money in circulation—the new processes, the novel forces which industry lays hold of, will add to the means of fraud unknown until the present time. Each of its members will find, we doubt not, in these few words, where we are not, nevertheless, able to express all our thoughts, a grave and profound motive to appeal to all its wishes, and to excite, as much as in it lays, the putting into practice those resolutions which have been for a long time elaborating, and which would have had the power already to place our money in a situation less dangerous for the country, and more in harmony with the actual state of the arts and sciences.

The details into which we are about to enter will render easily comprehensible, in fact, the new conditions in which commerce and the management of the precious metals will be placed, in the presence of an art which enables us to gild, to silver, or to platinize any metallic matter, and to spread them all in such a manner as not to alter the most delicate forms—of an art with which the object permits us to remake the mould, as with the mould it gives the means of reproducing the object; of an art, in fine, where the products are self-obtained, without noise, without preparation, without first expense, without workmanship, and where the least place will suffice for an extended execution of the process.

The Commission knows all the importance of these words ; it has maturely weighed them. But it was its duty to awake, now that it is time, and in the presence of an inevitable danger, the solicitude of the administration as well as that of commerce.

The gilding on brass and silver, that which is most practised, is constantly performed, and has been for some years, by means of mercury. After having carefully removed the verdigris from the piece, it is smeared over with an amalgam of gold ; it is then passed through the fire ; the mercury evaporating, leaves the gold on the surface of the piece. But in the practice of such a process, the workmen, exposed without cessation to the contact with the liquid mercury, or to the action of the vapours of mercury, are exposed in a high degree to the deleterious effects of the poisonous mercurial emanations.

The Academy has always taken particular interest, in the perfecting of this branch of industry, with regard to its effect on health. In 1818, a prize of 3,000 francs, founded by an old gilder on bronze, M. Raorio, was awarded to our colleague, M. D'Arcet, which at this epoch had not been received into its bosom by the chemical section. From that time the Academy has not lost sight of the art of gilding ; it has followed all the essays of which it has been the object, with the hope of there finding the solution of a question so worthy of the solicitude of all the friends of this class of artisans.

It is in this spirit that the Commission of the insalubrious arts has just proposed this year to the Academy to recompense the introduction into the arts of the galvanic gilding. The discovery of gilding by the humid method also, which is brought into practice on brass as much in England as in France, is there become the object of an important commerce, as a guarantee of its success and value.

The Commission distinguished the one from the other of these two processes of gilding, for the reason that the first, which depends on the employment of the pile, permits us to obtain the gilding of any thickness, and to gild all the metals, a fact which assimilates it to the process of gilding by the mercurial method ; whilst the second furnishes a thin gilding which is not really a good substitute for the gilding with mercury, and which in many cases cannot be applied to the same objects. However, it puts the artisans who practice gilding by the humid method to a scrupulous examination. It has studied the processes with care ; it makes them repeat and vary them under its eyes.

But at the moment when it was going to make known its opinion to the Academy, new incidents arose to complicate the question, by giving it proportions and an interest altogether unforeseen.

In fact, the Commission knew of the different publications of documents emanating from M. De la Rive, Professor of Physics, and Correspondent of the Academy, wherein this skilful philosopher made known the results which he had obtained in gilding executed

by means of the pile, in acting on solutions of the chlorate of gold. This process, of which the Commission had comprehended all beforehand, permits the augmentation at pleasure of the thickness of the lamina of gold; but it presents some real inconveniences, due to some difficulties in the execution, and to certain defects of adherence between the gold and the metal on which it is applied. The physical principle, the basis of the new art, once being discovered, it became necessary still to add thereto all the chemical resources necessary to render the gilding solid and brilliant, capable of taking the mat, the burnish, and the colours; in fine, it was necessary above all to render the operation economical.

The Commission were aware, also, of all that which concerned the process of gilding by the humid method, as it is practised by M. Elkington, whether in France or in England, and it had proved that this process could not replace, in a great number of cases, the mercurial gilding. In fact, by the humid method there can only be fixed a quantity of gold so small, and feebly fixed on the surface of the article, that it is impossible by the best gilding by the humid method to attain a thickness at which the worst gilding by the aid of mercury is forced to arrive.

Thus there remained some doubts in the mind of the Commission on the efficacy of the process of M. De la Rive in practice, though it appeared in its nature capable of fulfilling the object proposed by the gilding with mercury, and it remained convinced that, on his side, the process of M. Elkington did not replace the mercurial gilding, wholly constituting a new and very interesting branch of industry. The Commission had believed that they might conclude, from its essays, that the process of M. De la Rive gives a gilding sufficiently thick, but wanting in solidity and adherence; whilst that of M. Elkington, where the adherence is perfect, does not give a thickness which the objects well gilded by the aid of mercury attain. Different meetings of the Commission, to which the representatives of M. Elkington had been invited, have furnished the different members with an opportunity of expressing very plainly their opinion on this point, and no person made known any solution of the difficulty with which we had been preoccupied.

In the mean time, the Academy received from M. de Ruolz a memoir, in which were found descriptions of the processes in which the author, combining the employment of the pile and that of solutions of gold in the alkaline cyanurets, succeeded in obtaining on all the metals a gilding at the same time adhesive, solid, and of a thickness susceptible of being modified at pleasure, from pellicles infinitely small, up to laminæ of several millimetres in thickness. Generalizing his process, M. de Ruolz applied it to gold, to silver, to platinum, and to a number of other metals more difficult of reduction.

This memoir, and the products which accompanied it, had excited the lively interest of the Commission, when the agent of M.

Elkington, at Paris, became very desirous of submitting to the Academy a patent taken out by M. Elkington, and anterior by some days to that of M. de Ruolz. The Commission acknowledged, in fact, with surprise, that this patent existed, that it contained the description of a process for the application of gold having analogy with that of M. de Ruolz, and the Commission have not yet been able to comprehend for what motives he concealed the existence of the patent, which responded victoriously to all its objections, whilst at the same time it could not call in question M. de Ruolz and his processes.

This being the case, its duty was marked out; it was constrained to fulfil it. The proxies of M. Elkington have operated in its presence; M. de Ruolz likewise did the same. Each of them remitted to the hands of the Commission all the documents which they thought proper to be revealed; the analysis of these documents, the recital of their experiments, will put the Academy in a proper position for judging on the value of the processes of the two inventors.

We shall divide this report into three parts. The first is relative to the process by the humid method, such as it is practised in full by M. Elkington; the second will treat on the galvanic process of the same artist; the third, and last, has for its object the processes of M. de Ruolz.

1st.—Gilding by the Humid Method.

The gilding by the humid method is obtained by a very simple process in practice, but the explanation of which does not present itself in a very satisfactory manner to the minds of chemists, and which by that same fact, besides, ought to offer, and does offer, in fact, inexplicable irregularities in its employment.

This process consists in dissolving gold in aqua-regalis, which converts it into a perchloride of gold, and mixing this with a solution of bi-carbonate of potash in great excess, and then boiling the whole for a long period. The pieces of brass, bronze, or copper, well cleared from verdigris, are afterwards immersed in the boiling liquor, and the gilding takes place immediately, a portion of copper of the piece being dissolved to replace the gold which is precipitated.

In a note addressed to the Academy, an English chemist, M. Wright, has made known the results of researches undertaken by him, conjointly with M. Elkington, and from whence was derived a more satisfactory explication of the process than any which has been proposed hitherto.

It results, from their experiments, that the perchloride of gold is not precisely well adapted to the process of gilding; that the protochloride succeeds much better. They explain by that why it is necessary to boil the perchloride of gold so long a time with the

solution of bi-carbonate of potash, for during this prolonged ebullition the perchloride passes slowly, and with difficulty it is true, to the minimum. The liquor also takes a greenish hue; but the choice of the bi-carbonate of potash has a great influence on the result. This salt contains almost always some traces of organic substances capable of reducing the perchloride of gold to the state of proto-chloride. When the bi-carbonate of potash is too pale—when these organic matters are wanting, the operation is carried on with difficulty; whilst the presence of these same matters renders it very easy to conduct. Besides this, sulphurous acid, oxalic acid, the salt of sorrel, and many other organic matters or minerals, may be used to produce this effect, and there is nothing to prevent them from adding to the liquid, little by little, until the complete return of the gold to the inferior state of chloridation.

After these proper trials, your Commission is disposed to believe that the opinions of M. M. Wright and Elkington are established. It regards, then, the liquid employed in gilding by the humid process, as essentially formed of a combination of proto-chloride of gold and of chloride of potassium dissolved in a liquid, highly charged with carbonate and even the bi-carbonate of potash, which may be well understood if we consider the liquid as containing the prot-oxyde of gold dissolved in the potash, and supposing all the chloride to be in the state of chloride of potassium.

If experience should shew in future that the metals are precipitated better when we take their solutions in the same state of saturation as that of the salt which ought to replace them, the remark of M. M. Wright and Elkington will be of importance. They think, in fact, that the cause which ensures the success of the gilding by the humid method is, that the chloride of copper which is produced being a chloride of 2 atoms chlorine, we ought to employ a chloride of gold, also containing 2 atoms of chlorine, and not a chloride which contains a chlorine of 3, as is the case for the per-chloride of gold.

In conclusion, in order to appreciate the true character of the gilding by the humid method in the arts, it will suffice us to report here the analysis of different plates gilded, either by mercury or by the humid process, and proved, by the pains of our colleague, M. d'Arcet, at the laboratory of the mint. Plates formed of mixed metals, known in commerce under the name of *bronze*, were sent to different manufacturers, with orders to have them gilded. They endeavoured to obtain the strongest gilding, and that of the weakest description, still keeping, in all instances, within the limits of the usages of commerce.

The following are the results obtained from plates of 1 decimetre square :—

Quantity of Gold by the Square Decimetre in the Gilding by Mercury.

	By M. Plu.	By M. Denière.	By M. Beaupray.
	gr.	gr.	gr.
Maximum Gilding	0.1420	0.2333	0.2595.
Minimum Gilding	0.0428	0.0736	0.0695.

The quantity of gold in the two cases varied, then, in the relation of 100 : 16.5, or sensibly as 6:1.

We will, however, now give the results obtained by the humid method :—

Quantity of Gold by the Square Decimetre in Gilding by the Humid Method.

	By M. M. Bonnet and Villerme.	By M. Elombert.
	gr.	gr.
Maximum Gilding.....	0.0353	0.0422.
Minimum Gilding.....	0.0274	„

Thus, the best gilding by the humid method having fixed 0.0422 of gold to the square decimetre, and the poorest gilding by the mercurial method having taken 0.0428, we see that the gilding by the humid method scarcely attains, in the most favourable case, to the degree of thickness which the worst gilding with mercury is obliged to attain.

These are, then, two distinct branches of industry; the one cannot replace the other.

2nd.—The Galvanic Process of M. Elkington.

As this process is so very simple, and as its description is not very long, we will likewise give the text of the patent; here an analysis will suffice.

M. Elkington takes 31 gramme 25 centigrammes of gold, converted into oxyde, 5 hectogr. of prussiate of potash, and 4 litres of water. These must be boiled together for half an hour; at that time the liquid is ready to use. Boiling, it gilds very quick; cold, it gilds more slowly. In both cases, we immerse therein the two poles of a pile of a constant current, the object to be gilded being suspended at the negative pole, where the metal of the solution will be deposited.

In the patent of M. Elkington, the word prussiate of potash, which is employed without any other definition, leaves us in uncertainty, for chemists are acquainted with three prussiates of potash: the simple prussiate, the yellow ferruginous prussiate, and the red prussiate. The agent of M. Elkington, when pressed to explain this point, informed us that the patent intended the simple prussiate of

the cyanuret of potassium to be understood. In fact, when he submitted his process before us, it was the simple cyanuret of potassium that was made use of.

In the trials that we have made of the process of M. Elkington, we have gilded brass, copper, and silver.

In operating on a silver dessert spoon, with the liquid raised to 60° centigrades, we obtained a rapid and regular gilding. Scarcely was the spoon immersed ere it became converted into gold. During each minute there was deposited about 5 centigrammes, and we have not prolonged the experiments, but after six successive weighings we have found that the quantity remained the same for the same time.

We can, then, augment the thickness of the lamina of gold at pleasure, and calculate this thickness by the duration of the immersion.

But the simple cyanuret of potassium is an expensive salt, difficult to preserve in solution, to the employment of which will be raised divers obstacles in the manufacture, and it remains doubtful whether, in employing it, the gilding is performed to better account than by the actual method of mercury.

3rdly.—Galvanic Processes of M. DE RUOLZ, for the Application of a great number of Metals on other Metals.

As we have taken occasion to remark, previously, whilst M. Elkington solicited an addition to his patents, M. de Ruolz, on his part, took out a patent of invention for the same object. The patent of improvement of M. Elkington is dated Dec. 8th, 1840, that of M. de Ruolz is dated 19th December. Everything shews that M. de Ruolz has proceeded on his side without knowing the demand of M. Elkington; besides his processes are still very different to those of the English artist.

Leaving aside these questions of patent, which it is not our province to examine, and confining ourselves to the scientific discussion, we proceed to lay before the Academy the remarkable results obtained by M. de Ruolz.

Gilding.—In order to apply gold, M. de Ruolz employs the pile, like M. M. de la Rive and Elkington, but he has submitted to experiment such a variety of solutions of gold, that he has rendered it easy to find cheaper and more suitable ones than those made use of by M. Elkington himself.

Thus he made use of—1st, cyanuret of gold dissolved in the simple cyanuret of potassium; 2nd, of cyanuret of gold dissolved in yellow ferro-cyanuret; 3rd, of cyanuret of gold dissolved in the red ferro-cyanuret; 4th, of chloride of gold dissolved in the same cyanuret; 5th, of the double chloride of gold and of potassium dissolved in the cyanuret of potassium; 6th, of the double chloride

of gold and of sodium dissolved in soda;* 7th, of the sulphate of gold dissolved in the neuter sulphate of potassium.

Even chemists will be surprised to hear of these processes, of which the last-named—that which depends on the employment of the sulphurets—is the most convenient, and which, applied to gild the metals, such as bronze and brass, the sensibility of which is known in regard to sulfuration, it succeeds to the marvel, giving the most beautiful gilding, and the purest of tone.

The remaining processes succeed well, and the three latter, in particular, are applicable to all the metals used in commerce, and even on those metals which hitherto have not been employed in this manner.

Thus we can gild platinum, either on its whole surface, or on certain parts, so as to obtain designs in gold on a platinum ground.

Silver becomes gilded with great facility, so regularly, and with colours so pure and so beautiful, that the hope is permitted that at a future day all the vermillion will be obtained in this manner. The thickness of the lamina of gold may be varied at pleasure, and even its colour. A mixture of mat and polished gold may be obtained on the same piece. In fine, we may gild with equal facility objects of large dimensions, in plates or in relief, hollow or engraven, on wires of the thinnest and finest qualities. Of the samples laid before the Academy, we refrain from giving a detailed description in this regard.

All that has just been said in regard to silver, may be applied to copper, brass, or bronze. Nothing can be easier or more regular than the gilding of objects of different natures, which are fabricated in commerce with these three metals. Whilst gold, applied in pellicles excessively thin, constitutes a varnish sufficient to preserve them from oxydation; whilst, applied in thicker laminæ, it is capable of resisting, besides, all friction in its use; by a very simple artifice, the thickness of the layer of gold may be varied, so as to leave it thin in those places where the action of the air alone is feared, and to thicken it, on the contrary, where it is of importance to guard against the waste caused by friction against the parts. The jewellers will derive great advantage by these means, and science also will find her part in these advantages. Thus there is nothing to hinder us, in future, to gild, with little expense, all those instruments of copper which consume so rapidly in our laboratories, to procure us our tubes, capsules, and crucibles, of copper gilt, which will replace the vases of gold which are sometimes necessary, and which no chemist does not possess at the present day.

In fact, amongst the pieces deposited on the bureau of the Academy, there was a capsule of gilded copper, which had resisted very efficaciously the action of boiling nitric acid.

Packfong takes very well the gilding by this process, and it becomes

* The analogous salt of potash did not succeed.

easy to convert into vermillion the enamel in packfong, already scattered, and which are not without danger.

Steel and iron are gilded well and solidly by this method, though not in any relation, in this respect, with the old and very imperfect processes of gilding on iron or steel: it is only necessary to commence by putting on the iron or steel a cupreous pellicle.

Dessert knives, the instruments for the laboratory, the instruments of chirurgery, arms, the frames of spectacles, and numerous other objects in steel or iron, will receive this golden varnish with facility and economy. We have proved that divers objects of this nature have been received with lively satisfaction by the commercial world. The employment of gilded knives to common usage has caused us to observe, besides, that this application was of a nature to insist wearing, after being long in use, when the lamina of gold was only of a slight degree of thickness.

Pewter, under this relation, has been the object of the very interesting experiments of M. de Ruolz. He is convinced that it does not gild very well by itself; but when it comes to be covered with an infinitely thin pellicle of copper, by means of the pile and a cupreous solution, it becomes as easy to gild as silver. The silver-gilt pewter itself is of such a beautiful description, that it is certain that the commercial world well knows where to find profitable openings for this new product; it, however, becomes our duty to add, that by reason of the very high price of gold, it is difficult to put upon these covers of pewter a stratum of gold sufficient to render them durable, without too much raising the price of them.

The Commission has taken great pains to enlighten itself in a precise manner on these circumstances of the operation, by means of which gold is applied on the different metals. Different questions presented themselves. Can we, in fact, augment at will the thickness of the layer of gold, in such a manner as to produce the same effects as by the agency of mercury, or even in such a manner as to go further? Is the deposit of the metal made regularly, or in a variable manner? What was the part taken by the temperature of the liquid, of its concentration, of the number of elements of the pile, and the nature of the metals employed. Your Commission, without pretending to search into these questions, as they will be the subject of lengthened researches, are desirous, at present, to confine themselves plainly to treat them in a practical point of view.

1st. The precipitation of gold is regular; it is exactly proportionate to the time of immersion, a valuable circumstance, which permits us to judge of the thickness of the gilding by the duration of the operation, and to vary it at pleasure. In order to prove this, it will suffice to report here some of our experiments.

We operated on a liquid, containing 1 gramme of dry chloride of gold dissolved in 100 grammes of water, containing 10 grammes of yellow ferro-cyanuret of potassium.

The pile was charged with sulphate of copper and marine salt of 10° of weighed salt. We employed 6 elements of 2 decimetres on each side.

We operated at first on two plates of polished silver of 5 centimetres on a side; the surface requiring gilding was then 50 square centimetres.

Temperature of the Liquid, 60° cent.

		Gold deposited.
		gr.
First immersion of 2 minutes		0.063
Second immersion	„	0.063
Third immersion	„	0.063
Mean.....		0.063

Temperature of the Liquid, 35° cent.

		Gold deposited.
		gr.
First immersion of 2 minutes		0.028
Second immersion	„	0.028
Third immersion	„	0.030
Fourth immersion	„	0.029
Fifth immersion	„	0.027
Sixth immersion	„	0.029
Seventh immersion	„	0.030
Eighth immersion	„	0.030
Ninth immersion	„	0.029
Tenth immersion	„	0.028
Eleventh immersion	„	0.029
Twelfth immersion	„	0.027
Mean.....		0.0296

Temperature of Liquid, 15° cent.

		Gold deposited.
		gr.
First immersion of 2 minutes		0.009
Second immersion	„	0.013
Third immersion	„	0.014
Fourth immersion	„	0.014
Fifth immersion	„	0.013
Mean.....		0.0126

Thus, as we see, nothing is more regular than these numbers, the differences being probably attributable rather to the uncertainty of the experiments and of the weights than to the process itself. As

to the influence of temperature, it is manifest, and the rapidity of the deposit is augmented in proportion as the temperature of the solution is increased.

The nature of the metal probably exercises but little influence, provided it is a good conductor. The following experiment seems at least to prove it; it will also be confirmed by other instances.

We have gilded, in fact, a plate of brass of 5 centimetres a side, with the same elements, the same liquid, and in operating in precisely the same circumstances of temperature as for the plate of silver which served for our last experiment. We shall see that the weight of gold deposited is shewn to be exactly the same.

Plate of Brass of 5 Centimetres on a side.—Temperature of Liquid, 15° cent.

	Gold deposited.
	gr.
First immersion	0.010
Second immersion	0.013
Third immersion.....	0.012
Fourth immersion	0.012
Fifth immersion	0.013
Sixth immersion	0.012
Mean.....	0.012

We have remarked, in these kinds of trials, that the first immersion was often less efficacious than the following immersion. This circumstance is explained by the difficulty which is always experienced in preparing the metal so as to render it capable of being moistened on every part of its surface. Once overcome, however, this cause of error does not occur again in the succeeding proofs. Almost every one having explained it by an accidental circumstance, there remained some doubts on our minds, which we submit to philosophers. They will have to certify whether this peculiarity is not governed by a certain resistance on the part of one metal to deposit itself on another metal, which resistance disappears when it is only required to be deposited on itself.

In a word, in many of our essays, when gold, for example, was deposited on gilded plates, the weight of the deposit was always the same for a given time, whilst in the first immersion, where gold had to be deposited on silver or bronze, the weight of the deposit was less.

Silver Plating.—All that we have just advanced on the applications of gold, it is necessary to repeat in those of silver. M. de Ruolz has equally attained, by means of a cyanuret of silver dissolved in cyanuret of potassium, the application of silver with the greatest facility.

Silver may be applied on gold and on platinum, as a matter of taste or ornament.

It may be also very well applied on brass, bronze, and copper, in order to replace plate.

Pewter, iron, and steel may also easily be silvered.

The application of silver on copper or brass is accomplished with such a facility, that it is destined to replace all the methods of plating by hand, of plating by the humid method, and even, in many cases, the fabrication of plate. In fact, silver may be applied in thin pellicles, as it is practised to guarantee from oxydation a crowd of objects of ironmongery, and in coats as thick as it may be thought proper, in such a manner as to preserve them from wear. This is one of the applications which has most attracted the attention of your Commission.

For the purposes of chemists, we have proved that a capsule of brass, silvered, may be substituted for a silver capsule, as it has resisted at the point of fusion the action of hydriodated potash; a proof that it would not be necessary to repeat too often, since silver is dissolved in potash. From whence it evidently results that it will be of some interest to see how far we may extend the application of these new processes to the preservation of balances, to that of physical machines, to the preservation of the utensils employed in our households, among our confectioners, or of the apothecaries, and for all the preparations of aliments or of acid medicines.

Silver may be very well applied on brass. It furnishes also the means of driving away, with great facility, the disagreeable odour of covers of brass, by giving them, besides, the aspect and all the exterior properties of covers of silver. It will be, without doubt, one of the most important circumstances of the processes which occupy our attention, if in the place of brass, as the body of the piece, we can substitute another metal: we need only substitute another metal more economical and more solid. It acts upon iron, and even on cast iron. These metals, fashioned into covers and faced with a layer of silver, will become popular in France, by their easy sale; objects which are already common in England. There are manufactured, by other processes, much dearer and far less perfect, at Birmingham, many covers of iron silvered, and their use is common in most families in England. The experiment is then made, and the Commission has seen with the greatest interest the processes of M. de Ruolz furnish a plating, equal and perfect, on iron and cast steel, as the objects laid before the Academy prove.

Still recognising the fact that pewter may be silvered without difficulty, it seemed more convenient to the true interests of the finisher to make the covers of iron or cast metal silvered, and to reserve the silvered pewter for those pieces destined for less frequent using, and especially for the pieces obtained by the more delicate moulds.

Silver acts like gold when the solutions are reduced in the cyanurets, at least if we judge by the following experiments, where the

same pile was made use of as for gold, charged in the same manner, and placed in the same circumstances of temperature, but where we made use solely of 4 elements instead of 6.

The liquid employed for silvering contained 1 gramme of dry cyanuret of silver dissolved in 100 grammes of water containing 10 grammes of yellow ferro-cyanuret of potassium.

Temperature of the Liquid, 45° cent.—Plate of Red Copper of 5 Centimetres a side.

	Silver deposited. gr.
First immersion.....	0.007
Second immersion.....	0.013
Third immersion.....	0.012
Fourth immersion.....	0.013
Fifth immersion.....	0.013
Sixth immersion.....	0.013
Seventh immersion.....	0.012
Eighth immersion.....	0.011
Ninth immersion.....	0.010
Tenth immersion.....	0.010
Mean.....	0.0114

Temperature of Liquid, 30° cent.—Plate of Red Copper of 5 Centimetres a side.

	Silver deposited. gr.
First immersion.....	0.0055
Second immersion.....	0.0065
Third immersion.....	0.006
Fourth immersion.....	0.007
Mean.....	0.0083

Temperature of the Solution, 30° cent.—Plate of Brass of 5 Centimetres a side.

	Silver deposited. gr.
First immersion.....	0.008
Second immersion.....	0.007
Third immersion.....	0.007
Fourth immersion.....	0.007
Fifth immersion.....	0.009
Sixth immersion.....	0.008
Seventh immersion.....	0.008
Eighth immersion.....	0.008
Mean.....	0.0077

Thus, the same as with gold, silver applies itself with regularity, in weights proportionate to the duration of the immersions, and without the nature of the metals which we are silvering exercising any appreciable influence. This will nevertheless manifest itself in fact, but only at the moment of the first immersion, and it ought to disappear in the succeeding immersions.

As we might besides expect, the precipitation of silver is rather slower than that of gold.

To Platinize.—At first sight, after the analogy which exists between platinum and gold in many respects, we might have supposed that platinum is applied with as great facility as gold on the different metals already cited. The result, however, has presented serious difficulties for a long time, by the slowness with which it obeys the action of the pile. It was found necessary, with the solutions in the cyanurets, for example, to give to the experiment a duration of one hundred or two hundred times longer for platinum than for silver or gold of equal thicknesses.

But in using double chloride of platinum, and of potassium dissolved in caustic potash, we obtain a liquor by which we are enabled to platinize with the same facility, and the same promptitude, as when we are operating with gold or silver.

We will not insist on the very varied applications which platinum may receive in this new direction. The chemists will thus have the means of procuring large capsules of platinized brass, which will unite with cheapness all the necessary resistance to acid or saline solutions.

Armourers will put to profit, under different forms, this means of preservation of the oxydable or sulphurable metals which are used in the fabrication of arms; jewellers will thereby be enabled to introduce platinum in their decorations; those occupied in horology will find therein an excellent agent to cover with a very durable varnish those pieces the alteration of which they fear.

As the platinum, thus applied, may be obtained from the burnt solution of the ore of platinum, and as the metals which accompany the platinum are scarcely at all prejudicial to the process, we see that the platinum in this case costs hardly so much even as silver itself, for experiment proves that a thickness of the least moiety is a sufficient preservation. The result evidently is, that the uses of platinum, hitherto too numerous for the possible production of this metal, may be extended without limits, and will offer certain facilities for its use.

The fabricators of chemical products will have, without doubt, frequent occasions to avail themselves of the use of platinum under these new forms, and it will be very desirable, for example, if they can replace their cucurbites of platinum by those of iron platinized, used in the concentration of sulphuric acid. Where the use of cucurbits of glass is adhered to in many processes in the manufactures, they would be abandoned without doubt; thereby exposing, in the

least possible way, the lives or the health of their workpeople, if the apparatus in platinum should take a less-expensive form.

Apothecaries will find, in these new methods of employing platinum, occasion and means of putting, at a trifling expense, their instruments under shelter from a host of vexatious and prejudicial alterations.

In order to give a just idea of the difficulties which may result in these sorts of applications of the nature of the solutions that were made use of, we will here report some experiments.

We have made use of six elements of the same pile employed for gilding; they were charged in the same manner, and were operated with under the same circumstances of temperature. The liquid contained 1 gramme of cyanuret of platinum dissolved in 100 grammes of water, with the addition of 10 grammes of yellow ferro-cyanuret of potassium.

In fine, we operated at 80° to 85° temperature, at which the gold deposited was elevated to 0.030 gr. a minute at least. With platinum, the deposit obtained in one minute would be so feeble that it could not be appreciated. It was necessary to prolong these proofs to four minutes at least.

Plate of Brass of 5 Centimetres on a side.—Liquor of 85° cent.

Platinum deposited.

	gr.
First immersion of 4 minutes.....	0.001
Second immersion ,, 	0.001
Third immersion ,, 	0.001

Thus, in twelve minutes, a plate which would have received 0.378 gr. of gold under the same circumstances, only took 0.003 gr. of platinum.

These details will render appreciable and full of interest the observation of M. de Ruolz, who has recognized them; and as we have previously said, if we make use of a solution of chloride of platinum in potash, the deposit will proceed with the same rapidity as that of gold, or silver at least. In fact, if the precipitation of platinum could not be accelerated, the expense necessary to apply this metal would be augmented to such a point as to place its use within very narrow limits. It is desirable, on the contrary, that these should become numerous and profitable, on one hand, for the interest of the platinum mines, which have hitherto wanted a market, and on the other hand, for the interests of the consumers, who will find in the metals covered with platinum objects at once remarkable for their unalterability, their beautiful appearance, and the safety obtained in employing them for all the purposes of life.

The extraordinary extensibility of gold is well known; it has already attracted the attention of Réaumur, and of many other philosophers since the time this illustrious naturalist made known

his observations. But we might admit that platinum is not possessed of the same faculty, or at least that its extensibility is much less.

It is not, then, uninteresting to remark, that with one single milligramme of platinum, a surface of 50 square centimetres may be covered, which corresponds to $\frac{1}{100000}$ of a millimetre, analogous, as we see, to the utmost tension of which we can form a just idea by direct observation.

Coppering.—M. de Ruolz has not limited his applications to the precious metals. Extending his processes to all the useful metals, he has tried to copper, to zinc, and to lead, several of the metals in common use.

Coppering applied on iron plates or cast iron, gives the means of making, with greater advantage, the joists of ships, if experiment confirm the ideas which have been made on this product.

It is evident, in every case, that to iron plates, iron, the cast metal, natural or softened, may be communicated all the properties of copper, as far as it concerns the colour, polish, and the resistance to the air; and that by the nature even of the interior matter the low price of the product is thereby insured.

The coppering is achieved, like silvering, by means of cyanuret of copper dissolved in alkaline cyanurets; but the precipitation of copper is more difficult than that of the precious metals. For the remainder, that which we have just said of platinum shows how great the influence of the solution may be in this respect.

With eight elements of the pile already described, charged as in the preceding cases, and keeping in the same conditions of temperature, we have obtained deposits of copper much more feeble than if we had been operating upon gold or silver.

We operated upon one solution, however, which contained 1 gr. of dry cyanuret of copper for 100 gr. of the solution.

Temperature of the Liquid, 30° cent.—Plate of Silver of 5 Centimetres a side.

			Copper deposited.
			gr.
First immersion of 3 minutes			0.0015
Second immersion	„		0.0025
Third immersion	„		0.0030
Fourth immersion	„		0.0030
Fifth immersion	„		0.0020
Sixth immersion	„		0.0020
Mean.....			0.0023

Thus the copper, in being precipitated from its cyanuret, was deposited, like platinum, at the rate of 0.001 per minute for 50 square centimetres. This slowness will be, in practice, an obstacle, but which M. de Ruolz has no doubt previously studied.

In fact, the copper thus precipitated on iron will serve directly to preserve it, to give a beautiful appearance to objects of the locksmith's production, to balconies, balustrades, grates, chimney utensils, &c.

Besides, it can, we are assured, enable us to enclose iron in an envelope or case of brass. It is sufficient to make a deposit of copper and zinc on cast iron, then to heat the piece to a red heat in powdered charcoal. Brass is thus produced, and constitutes a metallic varnish less alterable than copper, and of a colour which we can vary at will.

In conclusion, at any time when we wish to go to the expense that the combustible which produces this latter operation exacts, we may be enabled to produce on the metals deposits in the alloys as easily as deposits of pure metal. This is a point of view which M. de Ruolz has not considered, but it is one to which we recommend his zeal and penetration.

The Precipitation of Lead.—In acting upon a solution of the oxyde of lead in potash, by means of the pile, we can lead iron plates, iron, and all the metals in general. The fabrication of chemical products will be benefitted by this discovery, by thus obtaining cauldrons of iron plates lined with lead, and where the solidity of the iron will be united to the resistance which lead offers to the chemical action of saline solutions and feeble acids.

In fine, there are but few circumstances in which lead obtains the preference over other metals, except for its low price and the facility with which it is managed. The new processes with which we are occupied will have, then, for their object rather to avoid the employment of lead than to increase it.

Tinning.—We have not yet said much of tin. The new processes may extend its applications, by giving an easy and prompt means of tinning copper, bronze, brass, iron, and even cast iron, by operating with it cold, and on all kinds of utensils.

Again, for a long time the workpeople who tin pins have, without knowing it, made use of a truly galvanic process, for they put among the pins grain tin, and water charged with cream of tartar. The two metals constitute a real pile, where the negative pole formed by the pins attract the tin to such a degree that it becomes dissolved, and being obliged to precipitate itself, the pins are thus covered with the tin.

The tinning of iron, as well as zinc, would be impossible by such a process; it is then necessary to have recourse to a real pile, independent of the metals employed.

On the contrary, in order to copper the metals which are negative with respect to tin, we can make a voltaic pair with the tin itself and the metal to be tinned, and by making use of cream of tartar to dissolve the tin, as is practical in the process of tinning pins, from a solution of the oxide of tin in potash, as proposed by M. Böttiger.

Cobaltizing—Nickelizing.—The Academy will remark with some interest the metallic pieces covered over with nickel or of cobalt among the specimens on its bureau.

Cobalt, whose colour approaches so near to platinum, has been employed in the recovering of musical instruments of copper, and it furnishes, in these cases, a metallic varnish agreeable to the eye, durable, and at a cost very little enhanced. However, all our experiments lead us to believe that platinum, gold, or silver, will obtain the preference; but cobalt will be able to find its place in such applications as a means of varying the tints.

Experience has proved, again, that by thus changing the surface of sonorous instruments, and by recovering the metals which form them with a coat of another metal, we do not modify in any point their properties, in a musical point of view. The most practised ear cannot recognize any change in this respect.

Nickel has especially been tried upon objects in the locksmith's branch, or in ornaments for harness. As it is not dear, and there does not require much to resist the air, it is well to note here that this metal may be applied very well on iron—a fact which may possibly become important in its application to locks for great security, to large clocks, to counters, and even to many parts of machines which we wish to preserve from the action of the air, without being obliged to grease them often.

Zincing.—Among the processes of M. de Ruolz, those which he applies to the zincing of metals, and of iron in particular, have highly interested your Commission.

The zinced iron acquires the faculty of resisting the oxydizing action of the air, and especially of humid air or of water. The fact is, that the zinc, which is more oxydable than the iron, preserves this metal from oxydation, and is scarcely at all oxydized itself; for when it becomes covered with a layer of sub-oxyde, all ulterior alteration is arrested.

In the greater part of the applications attempted by M. de Ruolz, the metal deposited is found on the contrary negative, with regard to the metal thus covered. All the guarantees which the metallic varnish promises in similar cases depends on its perfect integrity or connexion; for if it enters upon any point whatever, and the humid air can arrive at the interior of the metal, the superficial layer, far from being a preserver, will become, on the contrary, a determining cause of oxydation.

Zinc, applied on iron, preserves it doubly then, as long as it is in contact, like varnish, and when it is performed by a galvanic action. This particularity accounts for the success which the zinced iron, in all its applications where iron, or iron plates, being employed cold, have not lost any of their tenacity, and might support a supplement of expense.

In general, the zinced iron ought not to be employed for containing cold water; the galvanic action of the two metals determines

very rapidly the oxydation of the zinc, and the iron in its turn becomes red with a singular activity. This remark is intended, and ought to direct artisans to the employments they may make of the new processes, and is doubly sufficient to make them avoid mistakes except in very rare circumstances, but even then, less susceptible of being apprized of it by experiment alone.

The zincing of iron has hitherto been performed by immersing the iron in a bath of zinc, which is melted with some inconveniencies besides. The iron becoming attached to the zinc, thus constitutes a very brittle superficial alloy: the iron then loses its tenacity, a circumstance which is not at first perceived, until we attempt to zinc a very fine wire of iron or of iron plates very thin. Besides, the surface thus clothed with a layer of a metal slightly fusible, is always deformed.

Thus, by this process, we cannot zinc a fine wire of iron; it would become fragile and misshapen. Nor can we zinc balls or bullets; they would be misshapen, and no longer of the same calibre. The zincage of iron is not, by this process, applicable to objects of art: all their forms would be destroyed.

The industrial arts, the military art, the fine arts, will receive, then, with a high degree of interest, the processes of M. de Ruolz, by which is accomplished, and that economically, the zincing of iron, steel, and cast iron, by means of the pile with the solution of zinc; by operating with the solution cold, and in respecting, consequently, the tenacity of the metal; by applying it in very fine layers, and by the preserving of the general forms of the objects, and even the aspect of their smallest details.

There is, then, nothing to hinder us from zincing the iron wire employed in numerous ways, and which, far from rusting, will be preserved, at any rate, during many years, without doubt. Thus, the cords of suspension bridges and lightning conductors may be made of iron wire covered with zinc. We may say as much for metallic cloths employed in the fabrication of sieves, bolters, and of those which we apply in the construction of safety lamps. In the latter case, even the workmen charged in the mines with the care of cleaning the lamps, will be enabled, without sensible expense, to be supplied with everything that is necessary to restore the zinc, from time to time, without dismounting the lamp.

All the parts of machines, whose dimensions are so strong or so small as to render improper the zincing by the hot method, will be, on the contrary, susceptible of being easily zincd by the humid method.

The thinnest plate may receive this preparation without becoming cased; this will permit of the production of artificial slates of plate zinc perfectly applicable, and applicable with great economy, to the roofs of buildings.

The Commission were desirous of being convinced that it was possible to zinc cast-iron, and bullets in particular. It was certain

that this application would excite all the interest of the minister of war, and of that of the marine especially ; for the bullets alter so rapidly at sea, that their dimensions are soon modified in such a manner as to be prejudicial to the truth of the aim and to the duration of the pieces. It deposits a zinced bullet on the bureau.

In fine, the zincing of iron and of cast iron are of great importance to architecture and the arts of imitation. Every one knows with what promptitude the nails and bars of iron employed in buildings become oxydized, and consequently lose their tenacity ; and we all comprehend to what extent it is useful to preserve, with an eye to cheapness, all the pieces of iron disseminated in the walls of a building, for they are destined to give it a solidity which will become thereby durable and susceptible of being calculated with precision. In the same manner, grates and balustrades of cast-iron, receiving a coat of zinc in place of one of paint, which requires frequent renovation, will be thus found better guaranteed against the action of water or air.

It is especially desirable that these new means are called into use to preserve statues of cast-iron, of which we have recently seen the attempt in several of our monuments, and which, in some cases, have been submitted to the application of plasterers or painters, very ill calculated with regard to science, and with an effect very deplorable in reference to art.

The processes of M. de Ruolz for zincing may be applied not only on objects small and open, but it will be possible farther to make use of them for monuments in their place, and of large dimensions, by taking some precautions easy to foresee.

Your Commission is far from having sought to enumerate here all the applications which this new means of the zincing of iron is susceptible of presenting ; it has limited itself to those most essential, but they will very well suffice to make the Academy appreciate all the extent of the works of M. de Ruolz on this point.

Before quitting a subject so important, we will recall to mind the fact, that M. Sorel, on one hand, and M. Perrot, on the other, had already attained the art of covering iron with a coat of zinc by means of the pile, but still using, in all cases, solutions altogether different from those which M. de Ruolz believes to be preferable, and which have enabled him to act with economy, a point which is here truly important.

M. M. Sorel and Perrot have even announced, on this occasion, that they occupied themselves on the general problem of fixing the metals one on the other ; we will hope, that by making known their processes, they will add to the perfection of an art which already appears so far advanced.

The Academy will see, with the highest interest, an industrial art destined to spread itself in the world under all its forms ; to put to profitable use the pile of Volta, which had not hitherto been

industrially applied except at the metallurgic works of our colleague, M. Becquerel, and to galvanoplastik processes.

By the variety of his applications, M. de Ruolz gives to the pile an opportunity of multiplying itself and of spreading, which will become, we doubt not, a very certain cause of perfecting, both as to the construction of this apparatus, and as to the means of rendering it economic.

In conclusion, your Commission thinks itself obliged to declare that, forced as it has been to limit the time which it could consecrate to this examination, since it acted as a Commission for the prize Montyon, and as it could no longer withhold its report, it was bound to limit itself to trace here the summary history of these experiments, without pretending to make a systematic exposition of the state of the science on the point on which it has been occupied.

That which it has had in view is the economic application of it. All its researches have been in this direction: this was its duty.

Under this aspect, the experiments of M. de Ruolz have presented it with a character really novel. Their utility has appeared to him to be worthy of all the attention of the Academy. The Academy will be pleased to recollect also, that the author of these experiments has given proof, in this long work, of a remarkable penetration, and of a perseverance worthy of being crowned with complete success.

The Commission comes, then, to demand of you to decide with confidence that the memoir of M. de Ruolz be admitted to form a part of *Recueil des Savants étrangers*.

But it would require still more of you—and that in the view of public interest easy to comprehend—to decide that a copy of the present report be addressed to M. M. the Ministers of War, of Marine, of Finances, of Public Works, and of the Interior, who will be enabled therein to find tokens of a nature to interest the services of which the high direction is to them confided.

The conclusions of this report are adopted.

On the Powers of Large and Very Small Horse-shoe Electro-Magnets. By C. H. PFAFF, of Kiel.*

So long as the hope exists of bringing electro-magnetism into use as a motive power, there will be an interest instilled on experimental enquiries respecting the determination of the most favourable circumstances for the production of still more powerful electro-magnets. With the exception of the powerful electro-magnet described by the

* Poggendorff's *Annalen der Physik und Chemie*. Band. 52. s. 2. 1841.

American philosophers, Henry and En-Tyck, the lifting power of which was 2063 lbs., whilst its own weight was only 60 lbs., its length being 30 inches, when bent, and the diameter of the iron 3 inches, I am not aware of any being known in Europe whose lifting power exceeded 600 lbs. By the assistance of our skilful University Mechanic, Cramer, I find myself in possession of an electro-magnet, which, by the application of a powerful voltaic apparatus, not only suspends 900 lbs., but, what to me is the main point, I am thus also in possession of an apparatus which affords great facilities in comparing the lifting powers of electro-magnets under a great variety of circumstances. Indeed, the apparatus can be so arranged as to determine the differences to 0.001, nay, even to 0.0005 of the lifting powers, however great.

The apparatus, by means of which the lifting powers of the electro-magnets were ascertained, consisted chiefly of a lever, the length of whose arms was as *one to ten*; it was made of iron, and supported between two stout vertical pillars, the lower ends of which were morticed into a stout base-board, to which they were firmly fixed by means of additional spurs. It was supported by an iron wedge, with its sharp edge upwards, and which served for the fulcrum. This fulcrum was fixed to the pillars about half way up, so that the lever could play freely between them. To the extremity of the long arm of the lever was suspended a scale-board for the purpose of placing the weights upon; and the *cross pieces*, or *ankers*, that were employed, were attached to the short arm of the lever, the electro-magnet itself being fixed with its poles upwards. There were also some other appendages to this apparatus not necessary to describe in this place.

The large electro-magnet was so contrived that its influence could be easily ascertained under a great variety of circumstances. It is of soft iron, bent into the horse-shoe fashion. Its length outside is 3 feet 4 inches, and its mean length 3 feet $2\frac{7}{8}$ inches. It is formed of a cylindrical bar, whose diameter is 1 inch and $11\frac{3}{8}$ lines. It is of less diameter than the American electro-magnet in the proportion of about 2 to 3. Its weight is 32 pounds, and its branches are separated from each other to the distance of 2 inches $11\frac{1}{2}$ lines.

This electro-magnet is covered with 60 feet of copper wire, of the thickness of $2\frac{4}{16}$ lines, every 10 feet of which weighs one pound. It is wound round the magnet in six equal portions of 10 feet each, which form six distinct coils, three on each branch. Each extremity of these wires is furnished with a copper cup for holding mercury, by means of which, and short copper connecting wires, the coils can be so connected with each other and with the battery, that the current can either be divided into six distinct streamlets, one through each coil, or it can be transmitted entire and without division through the whole 60 feet length of wire constituting the coils. In the latter case, on account of the increased length of the

conductor, the resistance is proportionally greater, and the current consequently feebler: not, however, in the bare relation of the various resistances amongst the wires themselves, but must become so in the relation of these resistances with regard to the *whole resistance* offered to the current.

It is obvious that this arrangement of the electro-magnet at once affords the means of examining the effects of the currents when in operation on various parts of the iron. The electromotors employed in the following experiments were the well-known *spirals* of copper and zinc, the surface of each plate being 176 square inches, or the whole voltaic surface something more than $1\frac{1}{2}$ square feet. The exciting liquid consisted of water with $\frac{1}{7}$ of acid; half of which was concentrated sulphuric, and half aquafortis.

The spiral wires that surround the magnet are numbered from 1 to 6, commencing at that nearest to the end of one of the branches, and proceeding round the series, numbering them in succession to the other end; so that Nos. 1 and 6 are those spirals nearest the ends poles of the magnet, and Nos. 2 and 5 are those which occupy the middle places on each branch.

The first series of experiments with this electro-magnet were made whilst its ends were perfectly flat and smooth; so that the whole of both surfaces came into close contact with the flat smooth face of the cross piece, to which it clung by attraction. In the second series of experiments, the electro-magnet had its ends rounded in such a manner, that the flat face of the cross piece, when in contact with, and joining both poles, was a *tangent plane* to the curves into which the ends of the magnet were formed; and we shall presently see, with astonishment, the increase of lifting power gained by this means.

Experiments with the Flat Smooth Poles of the Electro-Magnet.

1.—One *spiral electromotor* brought a maximum of lifting power of $298\frac{1}{2}$ pounds.

2.—Two spirals, as an electromotor, of double the former surface, produced a maximum of lifting power of $368\frac{1}{4}$ pounds.

3.—Three spirals, as an electromotor of three times the first surface, produced a maximum of lifting power of $488\frac{1}{4}$ pounds.

4.—Three spirals, as a small battery, of three electromotors; each of which operated as a simple surface. Maximum of lifting power, $308\frac{1}{4}$ pounds.

5.—One spiral, as an electromotor, and the current transmitted through only half of the coils, viz., those three which were placed on one branch or shank of the magnet. The lifting power was $88\frac{1}{4}$ pounds.

6.—One spiral as an electromotor, and the current transmitted through coils 1, 5, and 6. Lifting power $58\frac{1}{4}$ pounds.

7.—Three spirals united into one, as an electromotor, with three times the first surface. The current, instead of being transmitted through the whole length of the coil wires connected in sequence, or end to end, was transmitted through each coil separately, or as a distinct conductor; so that each coil carried 1-6th of the whole flood. The lifting power was $98\frac{1}{4}$ pounds.

8.—One spiral as an electromotor: the current distributed as in experiment 7. Lifting power $28\frac{1}{4}$ pounds.

9.—One spiral as an electromotor, and the current transmitted through coils 1 and 6, each conducting one-half. Lifting power $98\frac{1}{4}$ pounds.

10.—One spiral as an electromotor, and the current transmitted through coils 2 and 5, or the middle coil on each branch of the magnet; each coil carried half the main current. Lifting power $88\frac{1}{4}$ pounds,

11.—Electromotor as in two preceding experiments, and current transmitted through coils 3 and 4, or those farthest from the poles of the magnet. Each coil half the main current. Lifting power, $78\frac{1}{4}$ pounds.

12.—Three spirals, as three electromotors, were employed in this experiment, and the lifting power at the first was $298\frac{1}{4}$ pounds. After the connections had been continued for some time, and without disturbing the exciting acid solution, the lifting power fell to $228\frac{1}{4}$ pounds. When the three spiral electromotors were connected so as to form one electromotor of three times the surface of each, the lifting power was 150 pounds; and after the circuit was opened, the magnet still carried $78\frac{1}{4}$ pounds.

13.—Three spirals, as three elements for an electromotor, were employed, The connections were such that each coil conducted $\frac{1}{3}$ of the whole current. The magnet, in one trial, would not carry the cross piece with the steel yard or lever, and, in another trial, not $28\frac{1}{2}$ pounds.

14.—When the currents from these three spiral electromotors were conducted through the coils in such a manner that Nos. 3 and 4 carried $\frac{1}{3}$, Nos. 2 and 5 carried $\frac{1}{3}$, and Nos. 1 and 6 carried $\frac{1}{3}$ of the whole, the magnet would not carry the anker or cross piece.

2.—*Experiments with the Electro-Magnet, when its ends or poles were rounded.*

15.—One spiral as an electromotor, and the current transmitted through the whole 60 feet, from end to end, of the coil wires. Lifting power, $618\frac{1}{4}$ pounds.

16.—Two spirals as one of double surface. The lifting power $878\frac{1}{4}$ pounds.

17.—Three spirals, as one of three times the first surface, produced a lifting power of $818\frac{1}{4}$ pounds.

18.—The same three spirals, as three electromotors (as a small battery), produced a lifting power of only $568\frac{1}{4}$ pounds.

19.—By repeated experiments, it was found that two spirals, formed as one voltaic element, produced a greater lifting power than three such spirals in one element; and these, again, a greater power than four such spirals joined into one element, - in the proportion of the following numbers: $728\frac{1}{4}$ lb., $668\frac{1}{4}$ lb., 648 lb.

3.—*Experiments with very small Electro-Magnets.*

In order to ascertain the excited lifting power of electro-magnets, in relation to their masses, we must employ, in the experimental inquiry, those only which are of small dimensions.

20.—An electro-magnet of thick iron wire, only $\frac{3}{8}$ of an ounce weight, covered with a double coil of thin silk-covered iron wire, lifted $3\frac{1}{2}$ pounds, or 300 times its own weight, when under the influence of three spirals as three electromotors, formed into a small battery.

21.—If, on the contrary, the same three spirals be united so as to form one electromotor of three times the surface, the same electro-magnet lifts only $2\frac{1}{2}$ pounds, or a little more than 213 times its own weight.

22.—A piece of iron wire weighing 24 grains, bent into the horse-shoe form, and covered with a double coil of iron wire, lifted 9 ounces, or 180 times its own weight, when under the influence of three spirals, as three elements. In this case, both the electro-magnet and the coils of wire became very hot.

23.—A similar electro-magnet, weighing only $\frac{3}{8}$ of an ounce, whose poles were half a line asunder, lifted only $3\frac{1}{2}$ pounds, or something more than 147 times its own weight, when under the influence of three spirals, as three elements.

24.—The same electro-magnet as used in the last experiment lifted only $2\frac{1}{2}$ pounds, when under the influence of the same three spirals as one electromotor of three times the surface.*

Results.

1.—The lifting powers of the electro-magnets, compared with their masses by similar electrical currents, are in the inverse proportion to their masses. Does not the structure, which is of a more filamentous character in the iron wire, act a considerable part in giving so much greater a proportional lifting power in the small electro-magnets over that shown by the thick iron bar, whose structure is of a more granular nature?

2.—By winding a copper wire of considerable thickness round the large magnet, where the resistance to conduction may be considered as nothing when compared with that presented by the electromotor, the power obtained will not gain by an increased number of elements without increase of surface; but, on the con-

* In all these series of experiments, it is to be understood that when the voltaic spirals formed one element, they acted as one pair of plates. In all other cases, they formed in series.—TRANS.

trary, much will be gained by an increase of surface only, until we arrive at a certain point. But the reverse holds good in those cases with the small electro-magnets and coils of thin wire, where the resistances to conduction have a sensible relation to those of the voltaic elements, as the experiments have shewn.

3.—That so little lifting power was produced in those cases where the current was divided into six portions, each of which carried one-sixth of the whole, compared with the lifting power when the current run through the whole length of the conducting coils in sequence, is easily explained upon the general laws of influences of the resistance to conduction, according to the strength of the currents. By experiments 7 and 3, the proportions were as $98\frac{1}{4}$ to $488\frac{1}{4}$, or nearly as 1 : 5. If the currents had been equally strong in both cases, the proportions of magnetic lifting power should have been as 1 : 6, because in the former case only 1-6th of the electricity operated upon each transverse section of the electro-magnet. The proportion of 1:5, of course, indicates to us the proportional strength which the currents had, in consequence of the coils offering channels of six times less transverse dimensions in one case than in the other. The diminution of resistance to conduction is, however, not to be understood as appertaining to the wire alone, but also to that produced by varying the arrangement of the voltaic elements themselves, which become less, as well as that in the wire, by the partition of the latter into several channels.

4.—It remains still further difficult to understand the results in experiments 16 and 17; where, by augmenting the surface of a single voltaic element, the lifting power of the electro-magnet, instead of becoming greater, as one would have expected, absolutely became lessened. Since it would be difficult to repeat these experiments under precisely the same circumstances, any attempt of that kind ought to be attended with great circumspection. It is my intention, however, as soon as leisure permits, to carry on another series of similar experiments under a more extensive point of view, and with such apparatus at command, as to be enabled to ascertain, with ease and certainty, the least difference of lifting power that may happen to be produced.

5.—There is still something very remarkable in the augmentation of lifting power by rounding the ends of the electro-magnet. It has been stated by a philosopher, that a cross piece, with a round face, will be more forcibly attracted than one with a plane or flat well-polished face. I cannot understand, however, how the flat face should weaken the attraction just one-half on each side: I should rather suppose, that amongst the whole of the attractions on the entire plane there results an equator, in which the tangent of the vortex of the curve falls, and from which the cause of the increase of magnetic power might be looked for, by considering that this equator can be drawn nearer in the magnet by rounding its ends, or by rounding the cross piece. If, for instance, the distance

of the equator vary from $\frac{1}{1000}$ to $\frac{1}{130}$ of a line, the lifting power would, from this cause, have its lifting power augmented about one-half.

I have above remarked, that even by raising the great lifting power of 700, 800, to nearly 900 pounds, the cross piece, when removed to about one inch from the electro-magnet, was not drawn to it, but required to be brought nearer by the assistance of the lever, before attraction took place. With respect to this fact, as well as for the general theory of magnetism, it appears to me that a foregoing series of experiments, which belong to Cramer, the mechanic,* deserve still further attention. Moreover, by a closer consideration, it will not appear strange that the reverse phenomena to those shewn in my experiments, viz., the repulsive power of similar magnetic poles, is of so little value.

* Cramer's Experiments will be found at p. 460. vol. vii.—EDIT.

ATMOSPHERIC ELECTRICITY.

Read by Mr. SPENCER, at the Polytechnic Institution, Liverpool.

SECTION I.

The immense number of meteorological changes that are attended with electricity have rendered the study of this agent absolutely necessary to the meteorologist. This, whenever attempted, has been rendered difficult, if not impossible, at the very outset, from the fact, that no two meteorological authorities hold opinions that are identical respecting the sources of this powerful agent. So much has this been felt, and so indefinite have been our ideas and speculations as to its origin, that my friend James Espy, of Philadelphia, in his recently published work, containing the labours and researches of years, has left electricity entirely untouched. This, at first view, might appear somewhat anomalous, and, to use a commonly quoted figure of speech, a work on meteorology without electricity might be considered as acting the play and leaving out the principal character.

The sequel, however, will show, I trust clearly, the justness of this gentleman's views as to the necessity of leaving electricity unnoticed until we have arrived at clear views respecting its proximate origin, or its functions, and the more especially, when we find another agent, whose laws and origin in the atmosphere we are able to subject to the strictest mathematical and chemical experiment, and one that is capable of accounting for nearly all the mightier changes that are constantly in operation.

In the beginning of the present year, the French Academy of Sciences were called to report on the justness of Mr. Espy's views on meteorology, and how far his theory was capable of explaining hitherto unexplained phenomena. After acknowledging the sound-

ness of this gentleman's views, being founded on experiments and known facts, they recommended the study of the action of electricity in the atmosphere as being the only thing wanting to complete the whole science of meteorology.

I had long regarded the phenomena of atmospherical electricity as a favourite speculation, but nothing more. As I saw no basis on which any theory could be grounded, I had abandoned the subject almost in despair. When Mr. Espy's views on the other phenomena of meteorology became known to me, I was at once struck with their truth and beauty, and from that hour felt that any labour or research I might subsequently devote to the study of atmospherical electricity would be attended, in all probability, with results much more satisfactory than those to which I had hitherto arrived. The result, I trust, will show, that these opinions were not unfounded; but, while I state this, I have much pleasure in acknowledging, that the researches of Mr. Espy, on those meteorological phenomena that are attendant on the development of latent caloric, have cleared away the ground, and have rendered my labour much lighter than it would otherwise have been.

Evaporation has ever been considered by meteorologists to be in some way or other connected with atmospheric electricity. Until recently, however, we had no precise notion as to the manner in which evaporation itself proceeds. It was held by some of the French philosophers, that this phenomena took place from the surface of the water in the form of invisible globular balloons, having an outward casing of water, the interior being filled with caloric, which latter agent gave them their buoyancy, and caused them to ascend in the atmosphere. The globules so elaborated, when deprived of their cargo of caloric, again descended to the earth in the form of rain, hail, and snow. However satisfactory this theory might appear to the mere mathematician, it failed to satisfy chemists, who are, in general, much better acquainted with the actual changes accompanying the laws of nature than any other class of philosophical observers. Still, the matter has remained unexplained, except by mere conjecture. One of our highest and most excellent authorities, Dr. Prout, says, that evaporation takes place because the molecules of *water* have a *tendency* to rise in the atmosphere. Chemically speaking, we are unable to understand how any imperceptible actions take place in forms of bodies by the mere tendency to do so, and without any further change or combination taking place. If we are to be told, that it is a law of nature that water should rise in the atmosphere from the surface of the earth at certain temperatures, our next inquiry would be, to ascertain whether it is a mere mechanical (if, under the circumstances, we can conceive such) or a chemical change. If the latter, it necessarily involves the phenomenon of a new substance. That it is not mechanical, will be sufficiently obvious when it is considered, that water absorbs about

1072° Fahrenheit of caloric before it will become what we somewhat erroneously term vapour. This, added to the 140° of caloric of fluidity already possessed by the water, will give us 1212° of latent caloric, which is held by this vapour in an insensible state, while the surrounding atmosphere may be 32°. It is clear, from this fact alone, that a *chemical* action takes place. Were it otherwise, the *heat* would remain *sensible*, as would also the *water*, and would *heat* and moisten any body passing through it. Indeed, such are the *only* proofs chemists ever require to ascertain the fact of chemical action having taken place : that is, whenever bodies that have entered into combination lose any portion of the properties they before held separately, or acquire new ones. Added to which, it in all cases occurs, that the result of such combinations are the production of *new* bodies possessing properties *totally distinct from either of their constituents*. The transparent aqueous vapour of the atmosphere, then, is precisely such a body.

The errors on the subject of evaporation have arisen from not always bearing this latter fact in view. So long as it was considered that water was evaporated *as water*, it is obvious that no very just conclusion could be arrived at.

It has also been proved, by experiment, that unless the air is agitated, no evaporation, even at a temperature of 180°, will take place. It therefore appears clear that the vapour does not arise in globules, but arises from the surface of water in thin *plates*. The result, however, of the process we term evaporation, is the formation of a highly elastic and transparent gaseous substance, which chemists may, one day, term a *hydrate of caloric* ; but we at present term it, when diffused in the atmosphere, *transparent* aqueous vapour, to sufficiently distinguish it from the small globules of water usually known as visible aqueous vapour. It will be seen that this body is the chief agent in the production of all those mighty atmospheric changes included in the science of meteorology. It is also the same agent that enables us to traverse the surface of the earth or water ; steam being precisely this transparent aqueous vapour, or gas, that we find diffused in the atmosphere, generated within the boiler of the engine, and exerting its elastic properties, and again losing them whenever it is resolved into water by *giving out* its caloric of elasticity.

We have now seen that a chemical change has been effected by the production of the elastic vapour ; but it also follows, that a chemical change is also produced whenever bodies are resolved *back again* into their former constituents. This proposition is as true as its converse.

It is now universally admitted by philosophers, that " all chemical changes are accompanied by electrical action." Here, then, we close with the subject, *all bodies* being more or less charged with this mysterious agent ; but it is only in what may, perhaps, be

termed its *free* state that it becomes evident to our instruments or senses. Among bodies *water* would seem to possess it in greatest quantity. Faraday and others have demonstrated, that there is as much electricity locked up *in a grain of water* as would cause an ordinary thunderstorm. The electricity contained in the water, apparently by the law of its composition, may, for convenience sake, be termed *its latent electricity*, and that which exists *on* aqueous surfaces, for the same reason, be termed specific electricity. When evaporation takes place at the surface of the water, *being, as we have seen, a chemical change*, the electrical equilibrium of this body is *disturbed*. According to experiments made by myself and others, this disturbance is of a decidedly *negative* character; but, as my object is more to account for the *real* or positive electricity that exists in the atmosphere, than to explain the phenomena attendant *on the absence* of it in the free state, usually termed negative electricity, I merely allude to the fact.

Let us now assume the evaporation as having taken place, and the elastic gaseous vapour diffused pretty equally throughout the atmosphere; but, for this diffusion, it does not require us to suppose that it possesses a superior levity to raise it, in the first instance, from the surface of the water, and then elevate it to the higher regions of the atmosphere. The currents of air that always agitate the water liberate the *thin plates* of the nascent substance from the surface.

We must now search for another law of nature to explain its diffusion, it being nearly alike over vast spaces.

Fortunately, in the present state of science, this is easily found. The "law of diffusion" that exists among *all* gaseous bodies, no matter what their specific gravity, first noticed by Dr. Dalton, and since investigated, in the most masterly manner, by Professor Graham, at once points out that the elastic gaseous vapour of water is governed by this law, *in common with all other gaseous bodies*. Were it to be governed by the laws of gravity, it would only be found in the higher regions of the atmosphere, it being 625—air being 1,000. This elastic vapour, then, fills the higher as it does the lower regions of the atmosphere.

Observation has shown us that the temperature of the atmosphere falls one degree of Fahrenheit for every hundred yards above the level of the sea, varying a little, more or less, with the climate and season.

Dr. Dalton and Gay Lussac have shewn, that when the dew point is 80° Fahrenheit, and the barometer stands at thirty inches, the quantity of elastic vapour held in solution in atmospheric air is nearly the 48th of its whole weight and 1-30th of its bulk.

Let us pause for a moment, and review what is already said. First, evaporation is constantly taking place from the surface of the water, and when we take into consideration the immensely large proportion of aqueous superficies possessed by our planet, it will at once

point out the immense quantity of water that is daily abstracted from its surface. I have also said, that water, when taken up into the atmosphere by means of evaporation, is water no longer ; but, by virtue of its combination with about 1,072 degrees of Fahrenheit of caloric, it becomes a gas possessing properties of its own, under certain circumstances possessing neither aqueous nor heating properties, just as we find in chemistry, that when an acid and an alkali combine they form a salt possessing *totally distinct* properties from *either* of their constituents. We have also seen, that, in accordance with the law of the diffusion of gaseous bodies, this substance must be pretty equally diffused throughout the atmosphere.

The experiments of Dalton and Gay Lussac prove this, and, indeed, those of all subsequent observers, to the average extent of 1-48th of its whole weight and 1-30th of its bulk.

These proportions are immense, and may show us the magnitude of the functions of this agent when mingled with the other gases of the atmosphere. I may here add, in parenthesis, that popular writers on physics, when speaking of the constitution of the atmosphere, will never convey correct notions on this subject, unless they enumerate aqueous vapour among the gases that constitute the air we breathe, it being of as much importance as any of the others, and, without it, it is difficult to see how any of the functions of animal or vegetable life could be performed.

It is true formula have been given by Mr. Daniels and others, by which we may calculate the amount of steam in the atmosphere, at certain barometrical pressures, combined with a given temperature, as thus, that the specific gravity of transparent aqueous vapour to atmospheric air is as 0.620 to 1.000. Suppose, then, we were to take 100 cubic inches of atmospheric out of a temperature of 60 Fahrenheit, and a barometrical pressure equal to 30 inches, we would find the whole to weigh 31 grains. But, suppose we wish to carry the inquiry still further, and find the actual amount of what is somewhat erroneously termed *dry* gas contained in the already given quantity of atmosphere, we should then find, that the 100 cubic inches had diminished in volume equal to 98.14 ; consequently, the transparent aqueous vapour, in such a quantity, would have a volume equal to 1.86 cubic inches. In like manner it would, also, be found, that the weight, 31 grains, had been reduced to 30.65, making the weight of 1.86 cubic inches of aqueous vapour equal to 0.35 grains. The term *dry*, applied to the other gases of the atmosphere, leads us to suppose, that the transparent aqueous vapour is really *wet*, when, in fact, such is not the case, as, before it will give out its *water as such*, a really chemical change must take place.

SECTION II.

Bearing in view the preliminary remarks on evaporation and the gaseous constitution of the atmosphere made in the former sec-

tion, we shall now enter more immediately on the subject of the electrical phenomena of the atmosphere, together with the explanations of many others which arise necessarily out of the adoption of the theory I have to propound.

It is admitted on all hands that the air is always charged, to a greater or less degree, with transparent aqueous gas, or vapour.

It is also admitted, that a column of atmosphere moving upwards, will lose, by diminished pressure, 1° of Fahrenheit for every one hundred yards it ascends from the earth.

Whenever transparent vapour is formed at the surface, the law of diffusion will cause it to ascend, perhaps aided—but on this I do not insist—by its specific gravity, it being only five-eighths of that of the atmosphere. The circumstances of the locality will, in some degree, accelerate or retard.

Whenever this vapour ascends high enough in the atmosphere, it will be cooled down, according to a fixed law, until it forms cloud.

The height at which this phenomenon takes place can be readily ascertained, at any given period, by the application of Dr. Dalton's great discovery of the dew-point; the height varying with the mean temperature and the amount of aqueous vapour contained in the air.

Whenever this change takes place, namely, the formation of cloud out of transparent aqueous gas, this body becomes decomposed, that is, its water, *as such, is set at liberty*, with about 1072° Fahrenheit of *latent caloric*, which was *before insensible*.

The caloric thus liberated expands the surrounding air enormously, and, from its levity, rushes upwards. This rush causes a corresponding movement in the outer circles of the atmosphere, and gives rise to one great section of meteorological phenomena, consisting of winds, storms, and tornados, well explained by Mr. Espy.

The water, which was before combined chemically in the gaseous state, is resolved into small globules of water, and forms what we term cloud. It is this cloud that furnishes materials for rain, hail, and snow.

When, however, this chemical change or decomposition of transparent aqueous vapour takes place, it is attended with another all-important, but hitherto unnoticed, phenomena, namely, the development of a *definite* and *necessary* portion of electricity, whose force can be measured with as much accuracy as that of the latent caloric developed during the same phenomenon. It is the electricity developed during this change that causes thunder-storms, lightning of every description, violent storms of hail, water-spouts, and aurora borealis.

When the latent caloric is abstracted from the water, were it not for the electricity developed simultaneously, this water would be precipitated at once to the earth, in sheets or streams, or irregular spouts, or in fog or mists, according to the quality liberated; and

we never should have any permanent cloud floating in the atmosphere *under any possible circumstances*.

The electricity thus developed attaches itself to the small globules of water when liberated, and surrounds their surface, giving them more or less tension, according to the amount of aqueous vapour present in a given space when the change takes place.

The surface of all the globules of water composing a given cloud having a like amount of positive electricity, are repelled from each other by the well-known law of electrical repulsion between similarly electrified bodies: the consequence is, that the cloud remains permanent, and is floated off by the next current, as an immense Leyden jar possessing an enormous amount of free electricity.

A cloud, formed under the circumstances I have stated, and floated off amongst others into the atmosphere, may be said to possess its natural portion of electricity; that is, that portion which forms one of the necessary conditions of its being a cloud at all. Clouds formed under those circumstances comprehend the aggregate of all the clouds formed; and it would not be too much to say, that, in our variable climate, for one storm-cloud there are 10,000 formed that are not so.

In tropical climates, or in our own in summer, when the air is surcharged with aqueous gas by the immense evaporation that has taken place, and when there are no disturbing currents of any magnitude, a *small* cold current in the upper regions of the atmosphere will commence the process of cloud forming, which, under those circumstances, takes place with fearful rapidity, and, from the size of a dark speck, expands itself into an immense black canopy, darkening the face of the atmosphere, and it is only by some disturbing current that it comes in contact with on the other hand that the process stops.

Clouds formed under these latter circumstances, it is obvious, will possess a much greater portion of electricity than those I have previously adverted to, because they are formed in an atmosphere containing an extraordinary quantity of aqueous gas, and the air, from a comparatively large circle, rushes to the spot; when discharged of its gas, the whole of the electricity liberated is contained in such clouds. These clouds, then, are the true storm or thunder clouds, because they possess more than what I have already termed their natural portion of electricity.

Let us now take a cloud formed under the circumstances just detailed, and one or a series of those formed under ordinary circumstances. In the first place, it will be evident to all electricians, and, indeed, to those having the slightest knowledge of this force, that clouds formed under the different circumstances named will be *positive* and *negative* to each other; that is, the cloud formed under ordinary circumstances will be negative as respects the cloud just termed the storm-cloud,—*the one will possess more electricity than*

the other. Let us now suppose, that the storm, or positive, cloud is floating in the atmosphere, and let it be borne in mind, that such clouds are generally higher or at greater distance from the surface of the land or water than the clouds formed under ordinary circumstances. This is said to arise from the air contained within the cloud being at a higher temperature than the surrounding atmosphere, and, in consequence of this rarefaction, their altitude is generally superior.

The storm-cloud is then carried by a current to the neighbourhood of a cloud that is negative with respect to itself. Whenever it comes within the sphere of its attraction, a discharge of the superabundant electricity takes place.

This discharge may take place under any of the following circumstances :—

First, the storm or thunder cloud may be a very large one, and, consequently, possess an immense quantity of free electrical force. If it comes in contact with a small negative cloud, a sharp clap of thunder, accompanied by a short shower of large dropped rain, will be the result, and the lightning given out will be by no means vivid. In summer, in our climate, such discharges often take place.

The small negative cloud that has just received the discharge from the large positive one now becomes in the same electrical state, and they roll off together. Both of the clouds now become positive with respect to ordinary clouds, but the larger positive one has diminished in bulk just in the ratio of the rain that has fallen, and a portion of the electricity of the cloud is carried to the earth or sea on the surface of the drops of rain. Thus agriculturists well know that a shower of rain will do more to *hasten* vegetation than nearly double the amount of water added to the soil by artificial means. This arises from the positive electricity, which, it is now well known, gives an immense impulse to vegetation.

Should the two clouds that have now become positive come in contact with a *series* of negative clouds, sharp, rattling peals of thunder take place like the, discharge of artillery, *seriatim*, and forked lightning is the result, accompanied by tremendous showers of heavy, large dropped rain. In tropical climates, under such circumstances, from the quantity of water held in solution in the atmosphere, the water descends literally in sheets. This rain, or rather what is usually termed the thunder shower, will take the direction of the discharge of electricity; that is, supposing the storm-cloud to be situated due north, and the end of a series of negative clouds to be south as respects the positive one, then rain will begin north and end south. During the continuance of a storm of this description, should the electricity or lightning pass off into the earth, or be conducted into it by any prominent object thereon, then the air will suddenly brighten: the dense black cloud will disappear, because it has been really annihilated, as a cloud, by the

electrical discharge that has taken place on the earth ; but, on the other hand, should the electricity not pass off to the earth, but be diffused through a mass of negative cloud, as soon as the transference takes place, this before comparatively-white cloud will assume the gloomy appearance of the primary thunder cloud, but never quite so dark to the eye, as a portion of the electricity was passed off to the earth with the rain. A thunder storm of the kind I have just detailed may be attended with another phenomenon which has been often set down as the echo or reverberation of the primary peals of thunder. This is caused by one of the most beautiful, yet no less wonderful, laws attending the movement or transference of this universal force. The idea of an echo or reverberation of sound is, when we take into view the materials (clouds) that are to reflect it, one that, on a little consideration, will appear to be quite untenable, and only tends to show the utter absence of the application of well known laws to explain these apparently enigmatical, but equally simple, occurrences.

To explain this, let us suppose that which often takes place according to the circumstances of the atmospherical currents, that another series of clouds are in the same stratum of atmosphere, and, at the same time, exactly opposite the series where the thunder-storm is taking place, and the direction of the discharge from *north* to *south*, as already predicated. The consequence of this state of things would be, that *another* series of *actual* discharges, accompanied with thunder, would take place in the other opposing series of clouds, but from *south* to *north*, and at the same time, much *weaker* in all the accompanying phenomena. This is caused by the law of electrical induction, which shows us, that no disturbance of electrical equilibrium can, by possibility, take place in one body, or a series of them, without causing a similar disturbance in any other conducting body in the vicinity. A series of clouds may be above or below the primary series, or they may be in almost any given position as relates to the primary, yet they will be necessarily disturbed by the first great discharges, and weaker peals of thunder will be the result, and at very opposite points of the compass.

All these various discharges will be attended with large dropped rain, and, if there is any echo or reverberation attending a thunder-storm, it must be in alpine districts, where the sound is re-echoed from hill to valley, and, even here, such must necessarily be of rare occurrence, as the peaks themselves serve as so many conductors that extract the electrical intensity from the clouds, while the water is descending in drizzling rain.

It is this attraction of the clouds to mountains that gives rise to many of the phenomena that influence the seasons in large districts, as on the Chilian and Peruvian side of the Andes we find a rainy season for many months of the year on the one side, while, at the

same time on the *other side of the same ridge* the land and vegetation is parched and dried for want of water.

With my present views, the explanation of this or similar phenomena becomes at once simple and easy. It may be said the Andes occupy the sea coast of one side of South America, with the vast Southern Pacific stretching out at their feet; while, on the other side, the immense plains of the Pampas extend for many hundred miles before they reach the Atlantic. In consequence of this relative position, as respects the ocean on the one hand and the land on the other, the mountain-peaks on the side next the Pacific attract the clouds that are formed by the water evaporated from its surface; but, from the height of the mountains themselves, they being much higher than that portion of the atmosphere usually occupied by clouds, rain can never get an opportunity to fall on the other side, because the clouds are really stopped in their progress and annihilated before they can by possibility reach it. This attraction of clouds by mountains has been long known and observed; but the true explanation is, *not* that the mountain attracts the cloud *per se*, but that the mountain, as a conductor of electricity, attracts the free electric fluid contained in the cloud to its vicinity. The cloud is, consequently, discharged of its electricity; then follows a precipitation of the water in the form of rain.

There are, occasionally, rare exceptions to this rule, where it is observed that the clouds are not always attracted by mountain peaks, and, when driven to their vicinity by a current, do not always discharge their rain. In such cases, the mountain is in the *same* electrical condition as the clouds, and, consequently, no action will take place in either body.

ROYAL VICTORIA GALLERY, MANCHESTER.

CONVERSAZIONE, JANUARY 6TH, 1842.

In Continuation of a Communication read on the 9th December, 1841.

BY C. W. WILLIAMS, Esq.

On the last occasion of my addressing this Institution, I explained the causes of the prevailing injuries to boilers, and proved that we were mistaken as to some of them. I shewed that the sediment in boilers assumes *two different forms*—namely, that of a chrystallised solid incrustation, and that of a loose mud-like body, held merely in suspension.

I proved that the *first* could not be the cause of injury, inasmuch as it was, of itself, a *good conductor* of heat; whereas the *second*—the floating matter—became a positive *non-conductor*, after the

boiler had been suffered to remain some hours at rest, and this matter allowed to subside and become dry and hard.

[Mr. Williams then exhibited several specimens of this sediment, and which had all the appearance of a solid mass, and as hard as baked clay.]

Since our last meeting I have pursued the subject, and feel confirmed in the statements then made. I then stated, that in *land engines* no injury could arise to the plates of boilers from anything which might arise from the furnaces, beyond the ordinary wear and tear, and where no artificial blast was used, if due attention were paid to cleanliness in the interior, and maintaining the water at its proper level.

Marine boilers, however, had other inconveniences to contend with, as exemplified by the models then exhibited. These were formed of a number of narrow vertical passages, in which the steam, as fast as generated, became so mixed up with the water, that a free circulation of the latter, and its continuous access to the heated side plates of the furnaces, was much obstructed. The parts thus most exposed to the greatest heat became most liable to be injured by the impediments which these narrow passages interposed to the free ascent of the steam, as fast as generated, and to the free access of the water to the plates—this latter being the result of the rapid ascending current of steam in those narrow passages.

Again, when we consider that these narrow passages, usually but five or six inches wide, (and not unfrequently as narrow as three inches,) have to conduct the steam from *two* furnaces, one on each side, it may well be considered doubtful how a sufficient supply of the recipient, *water*, could be preserved in continual and adequate contact with the plates.

I also observed, that in *marine* boilers bulging or rupture was not known, except where there was a deficiency of water, or, what was equally injurious to the plates, when, from the peculiar construction of the flues, the free circulation of the water was impeded.

I thus shewed, that this obstruction of the circulation of the water, by which, in point of fact, *steam*, instead of *water*, was most in contact with the plates, was equally injurious, in marine boilers, to the water getting below the proper level; that, in either case, the result was, that the *recipient*, or *absorber*, being changed from *water* to *steam*, the heat could not pass *as rapidly* through the latter as it was received by the metal; that, as already observed, the conducting power of the metal was therefore obstructed, accumulation of heat took place in the plate, and the inevitable result was, softening, expanding, bulging, and rupture.

I will now, observed Mr. Williams, give an illustration of these facts. I have here (exhibiting two models) one a vessel containing water, and to which the heat is conducted by an iron pin, and the other by a pin formed of incrustation; each three inches long, and

three-quarters of an inch in thickness. On the last occasion, I stated I had not ascertained which was the best conductor; having now tested them accurately, I found that the advantage was in favour of the iron, as seen by the table I now exhibit. The water being at 46 degrees of temperature at the commencement, in both cases, that in the vessel with the iron pin conductor reached the boiling point (212 degrees) in 13 minutes, while that with the incrustation conductor required $17\frac{1}{2}$ minutes.

These conductor pins, we see, were passed into the flame, and the great heat was transferred, *longitudinally*, through them to the water; yet the heat transmitted through that great length of incrustation (three inches long) was very nearly equal to that of the iron. That I consider quite conclusive on the point, that no injury to the plate could occur from the want of an adequate conducting power in the incrustation; for if heat can be conveyed with sufficient rapidity through incrustation three inches in length, we can have nothing to apprehend from such thickness as is met with on boilers, and which rarely exceeds one half-inch on those parts of the boilers where the heat was sufficient to injure the plate.

When these boilers were removed from the heat, these conductors were found to be at so low a temperature as to bear the hand being placed on them.

Mr. Williams then exhibited some diagrams, representing plans and sections of the first boilers of the "Liverpool," as forcible illustrations of the injurious tendency of these narrow passages, or water spaces, to obstruct the contact of the water with the iron plates. In that vessel's boiler these water spaces were above five deep, and but five inches wide, by which injudicious arrangement the steam, generated by the hottest plates forming the sides of the furnaces, was necessarily so great, in quantity, that the recipient might be said to be *steam* instead of *water*, and consequently that those plates necessarily became overheated, bulged, and ruptured.

Here a large plate taken from the furnaces of the "Liverpool" was exhibited, satisfactorily illustrating the above reasoning. This plate was bulged and twisted, manifestly from having been red hot, and exhibiting some large patches and cracks, proving that the plate must have been overheated in consequence of the absence of water to absorb the heat.

Mr. Williams proceeded to state that this fact had been fully established by an experiment made by the engineer, who inserted a trial pipe into one of the boilers, the inner end of which opened into the water space between two of the furnaces, and on the line of the fuel on the bars; the outer end projecting into the engine room, and furnished with a cock. On trying this pipe, when the furnaces were in action, the engineer stated that he never could draw off any thing but *steam*, and from the spot of all others which demanded the

uninterrupted presence of *water*; and this, although the place was some feet below the general level of the water in the boiler.

This, then, clearly established the point, and accounted for the overheating, bulging, and cracking of the side plates, which continued every voyage to exhibit proofs of similar overheating, and to require constant repairs, while the roofs and other parts of the furnaces remained sound and perfect, with but a single exception.

That exception occurred on the occasion of the blow-off cocks having inadvertently been left open: the consequence was that that section of the boiler, containing two furnaces, was entirely emptied—the roof and sides became red hot, and all collapsed. That section of the boiler had to be bricked up and thrown idle during the remainder of the voyage.

The point, therefore, to which I am desirous of directing attention (observed Mr. Williams) is this, that in seeking to protect the plates of boilers from injury, it is not to the fire, nor furnace, nor plates, our attention should be directed, but simply and solely to the *nature of the recipient* to which the heat is conveyed, *as on this rests the whole question of injury*.

Now this will naturally be cavilled at by those who have all their lives been talking and writing of the danger of hard firing and incrustation, and the want of due proportions in the fire and flue surface; yet I state the proposition broadly, that if we look to the *recipient and its heat-absorbing properties*, and attend to the *interior* of the boilers, we shall do all that is possible in the way of preventing injuries, except so far as regards over-pressure, but which we are not now considering.

Let us now inquire what are the several recipients of heat which present themselves in ordinary boilers; they are—

- 1—Water
- 2—Steam
- 3—Air
- 4—Incrustation deposit—crystallized
- 5—Loose deposit—uncrystallized

These are the recipients of heat which are to be met with in all boilers; but in *marine* engines there is no apprehension from the loose deposit on its subsidence, because it falls to that part of the bottom which is cool. Not so, however, in *land* engine boilers, where the lowest part, as in cylindrical boilers, is that which is exposed to the greatest heat, and hence the frequent cause of injury in the absence of internal cleanliness.

Thus we see, that so long as the water remains in contact with the plates, the latter can sustain no injury; since, being so excellent a recipient of it, the stream of heat (if we may so speak) is absorbed as rapidly as it is passed through the metal, and consequently the plate itself—the conductor—is unaffected beyond a certain point. What

that point is I trust I shall be able to demonstrate with sufficient accuracy for all practical purposes.

We saw that when we changed the recipient from *water* to *air*, the latter, being an extremely bad recipient, the stream of heat was interrupted, and accumulation necessarily took place. I shall hereafter be able to shew that the analogy between the stream of heat and the stream of water, through a tube, is sufficiently close to establish the fact I am contending for.

I will now illustrate this further by means of this model boiler and the table of temperatures, now exhibited.—[A small vessel was then shewn filled with water, having two conductor pins projecting one inch into the water, and three inches into the flame. Between these pins, and just between the boiler and the flame, and protected by a shield from both, the bulb of a thermometer was placed.]

Now, when these two bars (Mr. Williams continued) are exposed to the flame, the heat transmitted through them will raise their temperature to such a degree, that if they were *sentient beings*, they would tell us what they feel. This degree of heat will be indicated by the thermometer, the bulb of which is placed between them.

But I am desirous of establishing this fact, that the degree of heat which they sustain or *feel*, as mere carriers or transmitters, is very different from that which is *transmitted through them*; that there are, in fact, *two distinct temperatures* to be attended to—namely, the one which the bars or conductors may be said to *feel*, as *conductors*, the other that which they convey to the recipient, and of which the latter is susceptible.

Mr. Williams then explained a diagram, representing, on an enlarged scale, the model with the conductor pins, by saying the water in the boiler, at starting, was at 40 degrees. In five minutes the heat given out by the conductors to the thermometer was 160, the water being raised to 92. In fifteen minutes the water reached 212, and the conductor thermometer stood at 258.

Here we have two distinct temperatures, and two as distinct measures of heat. The first, which is that felt, as it were, by the conductors, I will call *statical* heat—that is, the heat due to its *status* as a conductor, and which, if it could speak, it would say “I feel.” The second is that measure of heat which would indicate the current or body of *conveyed* heat; this I will call *dynamic*, or power-giving heat. These terms, *statical* and *dynamical* heat, I use from want of others more appropriate, but they will sufficiently convey the distinction I am pointing out.

Now, to shew that there are those two temperatures, and which should be separately attended to, I give the following proof:—The bulb, indicating the *statical* heat, and which also indicates the temperature to which the conductor is raised, resting on the conductor

bar, stands at 270 (the water continuing boiling). To prove that this temperature is affected by the *nature of the recipient*, I change the recipient *water* for *air*. This is done by letting the water suddenly out of the vessel. What follows? The current of heat—the dynamical heat, cannot pass *out*, and *through*, as rapidly as it had previously done; it is therefore accumulated in the conductor—the statical heat is instantly increased—and the thermometer rises from 270 to 404, at which it remains.

Thus we see, that so long as the recipient is *water*, the *statical* or real sensitive temperature of the metallic conductor is 258; but with *air* as the recipient, the statical heat is 404.

Now, the *statical* heat is that which indicates the state of the conductor plates, and decides the whole question of injury to boilers. This is the practical bearing of the question. The temperature of the *plate* is then dependant, not on the quantity of heat, of *dynamical* heat passing through it, but on the recipient and the *rapidity* with which such dynamical heat is absorbed.

We have, therefore, two distinct classes of heat or temperature to attend to, namely, the statical heat—that due to the plate as a carrier, conductor, transmitter, or conveyer of heat; and the dynamical heat—that due to the quantity or current conveyed.

If the dynamical heat or current be taken up, or absorbed by the recipient water, the statical heat will never rise to a degree sufficient to have any injurious effect on the conductor plate. The statical heat or temperature depends on the rate at which the dynamical heat is absorbed by the recipient, and on this depends also the whole question, whether the plates which are the conductors can be injuriously affected or not?

I have now to draw attention to the commonly received notion of the durability of a boiler being influenced by the temperature within the furnace, and the activity with which the fire is urged. We are all familiar with the assertion, that a boiler will not last as long when subjected to *active firing*, and *more rapid evaporation* (as is usual under waggon boilers), as with slow combustion and slow evaporation, as is the case in the large Cornish boilers. This position I am prepared to dispute, and it appears to me to have originated in our not attending to the point I have just been enforcing, namely, the temperature of the plates, as conductors, and the causes of an increase in such temperature. Indeed, all the *facts* disprove this hitherto unquestioned *dictum*.

But there is one fact, which, if it stood alone, would disprove the allegation—namely, that those parts and plates which are most exposed, and subject to the *direct influence of the greatest heat*, are *the least affected*, and never give any indication of injury or deterioration, unless some accidental circumstances may have occurred to prevent access to the water.

Indeed, it has frequently occurred that plates taken from old boilers, and from the very part, above all others, most exposed to the violent action of the intensest heat in the furnace, are selected and put into other and even new boilers; being thus proved to be sound and uninjured.

Again, the plates which exhibit the greatest proofs of deterioration, are found to be those where the heat has been *the least*. These facts at once settle the question, that heat is not the *direct* or *immediate* cause of deterioration, and that we must look to some other source for it.

The whole question of destruction, practically, resolves itself into this, viz., that in land boilers, with quicker evaporation and more active firing, there is a greater average risk from the consequences of neglect in keeping the boiler clean, and the water at its due level; and in marine boilers, from the water being forced from its contact with the plates under the influence of rapid evaporation and ebullition.

It is true, neglect and mismanagement of the interior of boilers must be taken into account, and provided for. But why not endeavour to correct this evil, rather than, by attributing the cause of such evil to a wrong source, have our attention diverted from the true remedy.

Now, as a rapid action of the fuel, and a more intense heat, will give a larger evaporative effect, both as to *time* and amount of *fuel* employed, than slow combustion and slow evaporation with large boilers, it becomes a question of great importance, whether we should prefer the latter to the former—that is, the *inferior* to the *superior* process; for this would be paying dearly indeed for ordinary attention and cleanliness in the boilers.

What I desire is, to put the question, as regards the scientific and practical application of heat, on the right footing. But if we are to be deterred from giving utterance to our views because they are opposed to old rules and proportions, and because we are gravely told “operative engineers are opposed to such practice,” we had better at once proclaim that this is not the age of intellectual and scientific advancement, or ours the country for practical improvement. To this I am not disposed to assent, and trust to be able still further to develop the great advantage of combining science with practice, in all that has to do with the promotion of arts in this country.

GALVANOPLASTIK ; or the Process of Cohering Copper into Plates, or other given forms, by means of Galvanic Action on Copper Solutions. By Dr. M. H. JACOBI, Privy Councillor to the Emperor of Russia, and Member of the Royal Academy of Sciences of St. Petersburg.*

(Concluded from page 74.)

VIII.

In the preceding articles we have spoken of the reduction of copper on *copper* plates only, and have stated that the application of the galvanoplastik art would be very limited, if the process could not be extended to originals of other metals ; to all those metals, for instance, that would not of themselves decompose the copper salts ; that is, those which would not dissolve when placed in the cupreous solutions, and precipitate the copper therefrom so as partly to be covered by it. Those metals which sufficiently resist being attacked by the copper solution, are iridium, platinum, gold, silver, quicksilver, arsenic, bismuth, and antimony ; and, on the other hand, the copper solution becomes easily reduced by lead, tin, iron, and zinc. It may here be remarked, that pure and bright lead operates very differently to that which is tarnished. In the first condition it reduces the copper upon itself ; but when in the latter condition it scarcely acts upon copper solutions : so that tarnished lead answers almost as well as copper for receiving deposited copper by electric currents. Moreover, although pure lead corrodes a little at the commencement, that action very soon disappears, and in a short time a thin film of copper forms on its surface. By means of this metal, then, we may proceed at once to multiply engraved copper plates without the originals ever entering into the galvanic process. For this purpose a sheet of lead is laid over the original engraved copper plate, and both passed through a strong rolling press ; the lead is thus pressed into the engraving, and, when separated, presents a beautiful picture of it in relief. The leaden impression then serves as a form on which a fac-simile of the original engraving can be deposited by the galvanoplastik process. I have seen galvanic copper plates made by these means, which, for sharpness and accuracy throughout, were not surpassed by those deposited on copper plates made by the relief engraving machine.

Pure tin, by the action of cupreous solutions, becomes more corroded than lead, and thus presents an obstacle to its employment where the sharpness and purity of the original picture of the engraving is to be preserved ; but answers well enough where those particulars do not interfere with the object of the operator. The dissolution of tin discontinues, however, as soon as, either by che-

* Translated from the German edition.

mical or by galvanic means, it becomes covered with a film of copper. Alloys of tin, when this metal is not in excess, with those metals on which cupreous solutions have no action, even including lead, answer very well for patterns on which the copper is to be reduced; the proportions may very easily be determined by experiment. The type-metal, and also the well known fusible metal, are very applicable to this purpose.

It is well known that iron precipitates copper from its solutions until it has gradually and completely decomposed them, and the dissolution of the iron is not stopped by the formation of the loose copper covering that first forms on its surface. When the copper solution is saturated and contains free acid, the metallic copper falls down in the form of a powder; and although the precipitation is slower as the solution of the copper salt is more diluted, yet the metallic copper is so much the firmer cohered, that it very frequently becomes a firm mass, whose shape corresponds to that of the immersed piece of iron. The copper thus reduced by iron is well known by the name of *cæmentkupfer*, and is distinguished by its purity. In many places, where natural cupreous waters are abundant, this pure copper is obtained in great quantities. In Schmölnitz, in Upper Hungary, from 7,000 to 8,000 pounds of pure copper are obtained annually, by placing pieces of any kind of raw iron in the water naturally impregnated with the metal. I will here just remark, that this kind of chemical reduction as precisely differs from the galvanic reduction, as the chemical dissolution of zinc from its galvanic dissolution, which has already been alluded to in art. 1.

Zinc, as a model for galvanoplastik formations, is a metal that cannot be used, and it appears hitherto that its alloys with other metals are also exceptionable; such, for instance, is the case with brass. This alloy, it is true, by an interworking of galvanic currents, becomes overlaid, in a short time, with a beautiful film of copper; but it is always difficult, and often almost impossible, to loosen it even when grown to considerable thickness. But should we eventually succeed by the application of a strong force, we immediately perceive that the torn off piece of copper is covered over with a coating of brass, as though it were plated with that metal.

As regards the non-metallic bodies, with reference to their practical applicability, charcoal, and indeed in its form of graphite (plumbago), ought to be particularly mentioned. These substances will on no account affect the solutions of cupreous salts, and are capable of being employed to advantage, even better than platinum, in galvanic series, as cathodes in the galvanic decomposition of such solutions. Since, however, these substances present considerable difficulty in giving to them regular artificial forms, we must apply them in another manner; which, as we shall see, can be done to some advantage.

We have already seen that the galvanic action depends, principally, upon the *extent* of the metallic surfaces brought into a state of activity, and but very little on the thickness of the metal. From

this fact we easily understand that such non-metallic substances as will not dissolve in the liquid, if covered with a thin metallic film, or with a coating of graphite, will answer all the purposes of metallic originals.

Hence it is, that any non-metallic mould or pattern being so covered, and, by means of a suitable conducting wire made the cathode of a battery, will receive the reduced copper from the solution, which will cohere firmly together and form an exact metallic copy of the original from which the non-metallic mould was taken. The non-metallic substances which may be employed for this purpose, are clay or porcelain, slate, plaster of Paris, bees-wax, sulphur, stearine, sealing-wax, wood, &c., either in plates or any other artificial form. Any of these bodies employed as moulds or patterns are to have their surfaces covered with thin metallic leaf, or with soft impalpable metallic powders, or with powdered charcoal or blacklead; which, in order to prevent the fine lines and angles of the mould or pattern being blunted, must be laid on carefully and exceedingly thin. It will be obvious that these foils or powders can only be of such metals, as has already been observed, as will not of themselves chemically decompose the cupreous solution. We may obtain powders for this purpose ready prepared in the shops, or we may easily prepare them ourselves. For instance, we obtain a very soft powder of copper by precipitating it from its nitrous solution by iron. Besides the metals, we may also prepare plumbago in a finely washed impalpable powder, which is much finer distributed, and attaches itself to the face of the mould much better than any of the metallic powders. When thus prepared, it may be mixed with water and gently smeared over the model. When the film of plumbago has become perfectly dry, should there appear a superabundance on any part, it may be removed by a soft pencil or brush. The plumbago will adhere well enough to some substances without being moistened with water, and may then be laid on dry by means of a soft pencil, which, previously, has been made a little clammy with oil.

If the model or pattern be of plaster, it ought to be so prepared as to resist the introduction of the liquid for a long time. This property is best acquired by placing the plaster mould in a hot mixture of stearine and wax, and permitted to soak as long as air-bubbles are seen to ascend in this pure liquid. By this means the mould acquires a considerable degree of hardness, without losing any of the original sharpness of the design.

I have previously alluded to stearine as a substance which, when clothed with a metallic surface, can be employed in the *galvanoplastik* art. In again referring to this substance, I will here add, that when managed with caution, it produces castings from the softest design, which, for accuracy and sharpness, far excels any plaster casting whatever. My attention was first directed to this property of stearine by Count Alex. Bobrinski, to whose circumspect enterprising spirit, in spreading knowledge and promoting industry in his native

land, so many are grateful. The stearine, moreover, is susceptible of being well polished, turned, and, as it appears, of being engraved on—properties, in consequence of which, for this purpose, as well as in other respects, this material commands a high importance.

In the employment of non-metallic substances, some foresight is requisite in the application of the conductors which unite them to the zinc pole of the battery. However finely and equally the metallic powders, or the plumbago, may be distributed over the face of the mould, or pattern, they produce only an inferior conducting surface; and the reduction commences at those points only where this inferior conducting film is in intimate contact with the uniting conductor; and, consequently, the more we increase the points of contact, so much quicker will the surface of the mould be covered with reduced copper. I usually surround the model with a thin strip of rolled lead or copper, which easily bends to inequalities of the margin, and allows of being brought into intimate contact with the metallized or graphitized surface. It has still to be remarked, that the reduction proceeds with an uniform distribution, when we glue the original in a horizontal position, which position is particularly recommended, when the surface to be covered is of considerable dimensions, and in bold relief. All those parts which are intended not to be covered with copper—such as the connecting metal, the back part of the pattern, &c.—must be covered with wax, or other non-conducting substance, sufficiently to prevent any bare metallic parts being exposed to the cupreous solution; for, wherever there is an unnecessary deposit on such parts, it interferes with the action on the face of the model or pattern, and prevents the galvanic copy from arriving at the intended thickness. But we can, even under these circumstances, increase the thickness of the copper plate, especially where malleability is not a particular object. This is accomplished by strewing copper, in fine corns or filings, on the top of the first formed galvanic casing, which will very soon grow firmly to it, and become of the self-same piece.

IX.

The great variety of substances, partly plastic and partly otherwise manufactured, which can be employed in the galvanoplastik process, opens in this novel art, which may be considered as a *cold metallic foundry*, a field of infinite extent and interest, not only to the artist, but also in many other practical points of view. I will just point out a few instances of its practicability, which I have convinced myself of. Of the manner of copying engraved copper plates I have already noticed, and I will now add, that the same process is no less applicable to copying of medallions, coins, &c., whether of copper or of the noble metals. The galvanoplastik process can also be applied to the formation of matrices for single letters, or for entire stereotype plates; and again, the types themselves may be deposited in these matrices; or they may be used in the usual way as moulds for casting the letter type metal in.

Whether copper stereotype plates will deliver a greater number of sharp printed copies than can be obtained from the common type metal, is a question that can be determined by experience only. The pictorial stamps surrounding initial letters, and other typographical ornaments, and also those tools which bookbinders use, may, in future, be very easily made by the galvanoplastik process. The patterns may first be cut in wood, from which copper matrices may be taken; and from these again the required copper types will be easily obtained. Forms for tapestry and cotton prints are easily made by the same means, which affords an opportunity of their being procured in great variety, and to any extent. Maps and plans, *en relief*, of towns, have recently come into considerable repute, for the printing of which, the necessary forms and counter-forms may be immediately obtained by depositing the copper, to the required thickness, upon a proper mould or pattern made of any suitable material. Bas-reliefs, and other objects in relief in the arts, can be immediately formed in copper in infinite variety, upon wax embossed originals, even, perhaps, without the aid of the chisel; and, indeed, with all the sharpness and delicate softness that the original received from the hand of the artist.

You have already been made to understand that the first form for all these purposes can be taken in plaster moulds. This substance is also applicable, in a similar manner, for obtaining metallic forms or moulds for the manufacture of stone-ware pots, and the pottery ware which are decorated with relief figures. The galvanoplastik process, moreover, places in our hands the means of decorating our buildings, both interiorly and exteriorly, with cheap and numerous copies of the finest specimens of ancient bronzes; and, as it were, opens an entirely new and extensive field for the cultivation of one of the finest branches of the arts.

That objects composed of inferior materials may, by this process, whether for the protection against the weather or for other purposes, become covered with copper, is a circumstance worthy of mention in this place. By the galvanoplastik process, notwithstanding the accuracy with which we procure copies of original objects, we see that on the reverse side of the galvanoplastik copy the deposited metal assumes a granular appearance, and it is only when the process is carried on very slowly, and the layer of copper is left quite thin, that there is any appearance of the outside of the copy having any corresponding features with the original. Notwithstanding, however, the partial inaccuracy at which we can only arrive by this method, objects thus overlaid with copper, when seen at a distance, will still present to the eye the true figures and proportions. They may be covered with a metallic skin of any required thickness, and any bold protuberances or other accidental roughness that may form can easily be removed by the chisel.

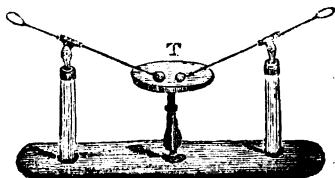
Hitherto I have treated only of such galvanoplastik processes as are applicable to obtaining simple copies from original forms or matters from which they are easily separated; for instance, from

engraved copper-plates, coins, medallions, seals, wood-cuts, &c. Were we to aim at the manufacture of entire figures, such as statues and other entire objects, for which a complicated mould, consisting of several pieces, would be required, the difficulties attending the process would be much greater than in those simple cases hitherto described: nevertheless, in the most complicated cases the difficulties that would probably be met with do not appear to be insurmountable. From a wax embossed bust, *en haut relief*, and much underworked, I have obtained a tolerably good galvanoplastik figure, though, it is true, not entirely free from blemish, in the following manner:—The original wax bust being well covered with plumbago, and furnished with a conductor, was submitted to the galvanoplastik process. When the whole had become covered with a thin skin of copper, the wax was melted out, leaving a hollow copper mould corresponding to the original wax figure. After washing out this mould with spirits of turpentine, it was again submitted to the galvanic action, as a cathode, in order that the precipitation might proceed within. When the process had continued long enough for the interior figure to be of the required thickness, the outer case, which had served as a mould, was loosened, and removed piece-meal, which, in some places, could not be accomplished without some degree of force. By these means, however, I obtained a true and perfectly smooth copy of the original, which in some few places only was a little deranged, from causes that may, in future, be avoided. It is true that by this method we lose both the original and the first formed metallic mould in every copy that we obtain; but it is to be hoped that this circumstance will be no hindrance to the diffusion of the advantages to be gained by the galvanoplastik art, from which so much may reasonably be expected, and in which process the workshops and laboratories of the artist and the handicraftsman will still be productive of many accomplishments and facilities which, as yet, can only be viewed in prospective.

ELEMENTARY LECTURES ON ELECTRICITY, &c.

LECTURE XIV.

Before proceeding any further in the experimental department, it will be necessary that I explain to you some other pieces of apparatus that will be wanted as we pursue our illustrations. Here we have a very beautiful, and, for many experiments, an exceedingly useful piece of apparatus, called the universal discharges.—(See fig. 1.) A mahogany board constitutes the base of this instrument. Near to each end of this board is fixed a vertical glass pillar, surmounted with a brass piece, consisting of a socket for the introduction of the upper end of the pillar, an universal joint, and a small spring tube for



the reception of a brass rod. The brass rod moves freely in the tube, and is furnished with an opening at one end, and a small brass ball at the other; the latter screws on to the wire, which is pointed, and is occasionally removed for certain experiments in which points are required. From the centre of the base board rises a short wooden pillar, which carries a small table, for the purpose of placing on it certain objects of experiment. If, now, I raise the balls of both wires from the table till their wires are nearly horizontal, and place the balls at about an inch apart, sparks, which are taken from the prime conductor to one of the rings, will be seen darting between the two balls, and will pass off invisibly to the ground, through the other wire, and such other intermediate conductors as form its uninsulating connection. If the balls be brought closer together, the sparks will be smaller and more frequent; and if they are removed further asunder, the sparks will be more powerful and brilliant, will make a greater noise, but will pass less frequently than before.

If, now, I unscrew both balls of the brass rods, and expose the points to one another, we shall have a very beautiful display of the *brush* and *star*; the former issuing from the wire in connection with the prime conductor, and the latter on the point of the uninsulating wire.

By means of this convenient piece of apparatus, many of those experiments shewn in our last lecture are very easily and more elegantly performed. To perforate cards and other solid bodies, for instance, they may be very conveniently placed between the balls of the universal discharger, with their lower edges resting on the table of the instrument, and gold leaf placed between pieces of glass is easily prepared for the reception of an electrical discharge by means of this instrument. As I have another fact to notice in the perforation of cards, we will employ the universal discharger in the experiment.

I will place a card vertically between the balls of the universal discharger, which do not press firmly against it, but merely prevent it from falling whilst its lower edge rests on the small table. Thus arranged, I discharge the jar through the card, and the usual perforation is made; but by close observation you will find that the card, about the edges of the hole, is protruded on both sides, as if the electric fluid had passed in opposite directions. There are but very few experiments in electricity that have made so much noise as this and its trifling variations. It has been the means of leading philosophers to very different opinions respecting the cause of the two burs on the opposite sides of the card, and has, consequently, been a subject for much ardent discussion. Dr. Franklin was the first person who pierced a card by an electrical discharge, but the protrusion of the card on the opposite sides was first observed by Mr. Symmer; indeed, on one of the cards which had been pierced by Dr. Franklin's experiments. Mr. Symmer made many experiments of this kind, varying them in many ways, all of which produced similar effects, and led this electrician to infer that there are two

opposite electric fluids dependent on each other ; that they may be uncombined by the power of the machine, and occupy the opposite sides of a jar or any other charged glass ; but by means of a conducting communication, they avail themselves of the opportunity of reuniting with a promptitude unparalleled by any other kinds of matter, and thus, whilst travelling in opposite directions, produce all the effects of electric forces, the double protrusion of the card being supposed to be an instance of the effects of their opposite actions.

If we vary this experiment by perforating several plies of writing paper instead of the single card, we find that the protrusion of the paper from the centre of the pile is in opposite directions, and that no individual ply of the paper has the protrusion, or bur, as it is often called, on both sides. Many attempts have been made to explain this phenomenon, though, it must be acknowledged, not very satisfactorily. Those who advocate the existence of two electric fluids think their views triumphantly demonstrated by the double bur experiment, whilst others consider that expansion is the real cause even of both burs, which is by far the most probable view, though it generally happens that the bur or protrusion on the positive side is smaller than that on the negative side.

If, instead of a card or several plies of paper, we were to set up a piece of tin-foil between the balls of the universal discharger, a discharge from the jar will perforate the foil. But any of these articles can be much easier penetrated when the expansion of the electric fluid is more limited, than whilst passing through the atmospheric air ; that is, by surrounding the interruption with more dense fluids, such as oil, or even water.

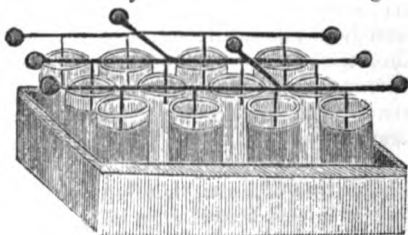
If we immerse a pointed wire, bent as in the experiment with the phial of oil, to some depth in a tumbler of water, and opposite to the point place a thin piece of metal, such as thin sheet brass, then by passing a series of sparks from the prime conductor through the pointed wire to the thin metal, the latter will become perforated by the mechanical action of sparks which passed from the point. When the sparks are passed through oil instead of water, they are still more compact by a less lateral expansion, and accomplish the perforation sooner. The lateral forces, however, in all these cases, are still considerable, and mechanical effects are produced by them as decidedly as by the direct action.

It must be here noticed, however, that if a bullet be discharged from a gun through a sheet of copper, effects are produced analogous to those exhibited by the card or paper by an electrical discharge. I have lately received from my friend, Mr. Marsh, of Woolwich, two penny pieces which were shot at whilst flying in the air ; and they are both pierced by the leaden shot ; one of them by a bullet from a rifle, which has taken a piece out of the side of the coin, to the depth of little more than half a diameter of the ball, leaving a bur on each side of the metal, round the crescented margin of the semi-lunar breach in the edge of the coin. The side which was hit by the

bullet is easily known by the copper being bent by the blow ; and the bur on that side is far smaller than that on the other or off side. The other penny has been struck by seven small leaden shot—perhaps the size of duck-shot—four of which have pierced the coin, and the other three have made deep indentations, but not passed through. In this, as decidedly as in the former case, the bur occurs on both sides, but less on the near side, or that which the bullet first struck, than on the other.*

In many electrical inquiries experimenters have found it necessary to augment the force beyond that which can be accumulated by a single jar, however large ; which has given rise to the formation of the electrical battery, consisting of several jars, so arranged as to have the whole of their inner surfaces united by metallic conductors, and all their outer surfaces also united by similar means. In fig. 2,

we have a battery of this kind very accurately represented. The battery consists of twelve jars placed in a mahogany box, whose inside is lined with stout tin-foil ; which, of course, comes into intimate contact with the metallic coatings of the whole series of jars. From the inside of each jar rises a stout brass wire, and the twelve jars are divided into three groups of four each, by three brass rods, each of which unites the interior of four jars ; and, by means of two cross wires or rods, the groups of jars can be united with each other ; so that, by this contrivance, we have an opportunity of employing either four, eight, or twelve jars, as we please. The horizontal rods are terminated with well polished brass balls, to prevent loss of fluid.



When we employ the battery of jars, we bring one of the horizontal rods into connection with the prime conductor, and it is obvious that, by these means, the whole of the jars will become charged at the same time ; and by connecting one of the branches of the discharging rod with a chain in connection with the lining of the box, and approaching any of the rods with the other branch, the discharge of the whole will take place. There are, however, many precautions to be taken in the employment of a formidable battery, some of which it may be necessary to point out before we proceed to experiment with it.

* See Mr. Marsh's letter in this number.

THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND
CHEMISTRY;
AND
GUARDIAN OF EXPERIMENTAL SCIENCE.

MARCH, 1842.

*Historical Sketch of the Production of Electricity by Evaporation,
and of Evaporation by Electricity.*

The rage for steam electricity having now become greatly abated, we will endeavour to collect such theoretical opinions concerning the cause as we can find in the journals of the day; and also those which were entertained many years ago.

We are not certain that Dr. Franklin was the first philosopher who entertained the idea of the electricity in clouds being taken up from the earth in its aqueous vapours; but such was his opinion, however, as early as the year 1749, as is clearly stated in his fifth letter, which is highly probable to be the first time that such an idea received publicity. From the results of many experiments on atmospherical electricity, Franklin was led to suppose that the generality of thunder clouds are negatively electrical with respect to the earth. Hence he says, in his twelfth letter, "*For the most part, in thunder strokes, it is the earth that strikes into the clouds, and not the clouds that strike into the earth.*" And in order to give an explanation for this negative electric condition of clouds, he proceeds thus:—

"I conceive that this globe of earth and water, with its plants, animals, and buildings, have diffused throughout their substance a quantity of the electric fluid, just as much as they can contain, which I call the *natural quantity*. That this natural quantity is not the same in all kinds of common matter under the same dimensions, nor in the same kind of common matter in all circumstances; but a solid foot, for instance, of one kind of common matter, may contain more of the electric fluid than a solid foot of some other kind of common matter; and a pound weight of the same kind of common matter may, when in a rarer state, contain more of the electric fluid than when in a dense state. For the electric fluid

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being attracted by any portion of common matter, the parts of that fluid (which have among themselves a mutual repulsion) are brought so near to each other by the attraction of the common matter that absorbs them, as that their repulsion is equal to the condensing power of attraction in common matter; and then such portion of common matter will absorb no more.

"Bodies of different kinds having thus attracted and absorbed what I call their *natural quantity*, i. e., just as much of the electric fluid as is suited to their circumstances of density, rarity, and power of attracting, do not, then, shew any signs of electricity among each other; and if more electric fluid be added to one of these bodies, it does not enter, but spreads on the surface, forming an atmosphere; and then such body shews signs of electricity.

"I have, in a former paper, compared common matter to a sponge, and the electric fluid to water; I beg leave once more to make use of the same comparison, to illustrate further my meaning in this particular. When a sponge is somewhat condensed by being squeezed between the fingers, it will not receive and retain so much water as when in its more loose and open state. If *more* squeezed and condensed, some of the water will come out of its inner parts and flow on the surface. If the pressure of the fingers be entirely removed, the sponge will not only resume what was lately forced out, but attract an additional quantity. As the sponge, in its rarer state, will *naturally* attract and absorb *more* water, and in its denser state will *naturally* attract and absorb *less* water, we may call the quantity it attracts and absorbs, in either state, its *natural quantity*, the state being considered.

"Now, what the sponge is to water, the same is water to the electric fluid. When a portion of water is in its common dense state, it can hold no more electric fluid than it has; if any be added, it spreads on the surface. When the same portion of water is rarified into vapour, and forms a cloud, it is then capable of receiving and absorbing a much greater quantity: there is room for each particle to have an electric atmosphere. Thus water, in its rarified state, or in the form of a cloud, will be in a negative state of electricity; it will have less than its *natural quantity*—that is, less than it is naturally capable of attracting and absorbing in that state. Such a cloud, then, coming so near the earth as to be within the striking distance, will receive from the earth a flash of the electric fluid; which flash, to supply a great extent of cloud, must sometimes contain a great quantity of that fluid."

Again, our philosopher states:—"Thus thunder-clouds are generally in a negative state of electricity compared with the earth, agreeably to most of our experiments; yet, as by one experiment we found a cloud electrized positively, I conjecture that, in that case, such cloud, after having received what was, in its rare state, only its *natural quantity*, became compressed by the driving winds, or some other means, so that part of what it had absorbed was forced out,

and formed an electric atmosphere around it in its dense state. Hence it was capable of communicating positive electricity to any rod."

The principal object for bringing forward these quotations, is to shew that, in the opinion of this great philosopher, the aqueous vapour rising from the surface of the earth is *negatively electrical* with respect to the natural electrical condition of the water from which it ascended. There are, however, other topics of high theoretic importance contained in these few paragraphs. One of the great beauties in Franklin's theory is, that, under natural circumstances, each peculiar kind of matter is possessed of a *specific quantity* of the electric fluid, an idea parallel to that of Dr. Black, respecting the *specific heat* of bodies; and may eventually, perhaps, become of parallel practical importance. In support of this part of his theory, Dr. Franklin invented a very interesting experiment with a can and chain, now well known amongst electricians. A brass chain, of some few yards long, was coiled in a silver can, and a certain degree of electric action, measured by an electrometer, was given to this apparatus by a spark from a machine. One end of the chain was now lifted up, by a silk thread previously attached to it, until most of the chain was out of the can. With this increased surface the electric tension became much lessened; but when the chain was again let down into the can, the electric tension increased almost to its original degree.

"Thus," says Franklin, "we see that increase of surface makes a body capable of receiving a greater electric atmosphere. But this experiment does not, I own, fully demonstrate my new hypothesis; for the brass and silver still continue in their solid state, and are not rarified into vapour, as the water is in the clouds. Perhaps some future experiment on vapourized water may set this matter in a clearer light. One seemingly material objection arises to the new hypothesis, and it is this. If water in its rarified state, as a cloud, requires, and will absorb, more of the electric fluid than when in its dense state as water, why does it not acquire from the earth all it wants at the instant of its leaving the surface, while it is yet near, and but just rising in vapour?"

In this doubtful position, we believe, Dr. Franklin left the subject, and it was not till many years after that Volta set the matter somewhat at rest, by producing electric action on the conversion of water into steam. In Franklin's 59th letter, dated Paris, 1767, we find that he has changed his opinion respecting the electricity of the clouds; for he there says—"The clouds have often more of this fluid in proportion than the earth; in which case, as soon as they come near enough (that is, within striking distance), or meet with a conductor, the fluid quits them and strikes the earth."

About the year 1752, the Abbé Nollet pursued a series of experiments on the evaporation of water and other liquids by electric agency. The following were the results:—

" 1. Electricity augments the natural evaporation of fluids ; since, excepting mercury, which is too heavy, and the oil of olives, which is too viscous, all the others which were tried suffered a diminution which could not be ascribed to any other cause than electricity.

" 2. Electricity augments the evaporation of those fluids the most which are most subject to evaporate of themselves, for the volatile spirit of sal ammoniac suffered a greater loss than spirit of wine or turpentine ; these two more than common water ; and water more than vinegar or the solution of nitre.

" 3 Electricity has a greater effect upon fluids when the vessels which contain them are non-electrics ; the effects always seeming to be a little greater when the vessels were of metal than when they were of glass.

" 4. This increased evaporation was more considerable when the vessel which contained the liquor was more open, but the effects did not increase in proportion to their apertures ; for when these liquors were electrified in vessels, whose aperture was four inches in diameter, though they presented to the air a surface sixteen times larger than when they were contained in vessels whose aperture was one inch in diameter, they were, nevertheless, far from suffering a diminution proportioned to that difference.

" 5. Electricity does not make any liquors evaporate through the pores either of metal or of glass ; since after experiments, which were continued ten hours, there was found no diminution of their weight, when the vessels in which they were contained were well stopped."

(On these experiments of the Abbé Nollet, Beccaria remarks, "The Abbé Nollet has found, by accurate experiment, that the electricity *without sparks* promotes the evaporation of liquids proportionally, not so much to their surface as to their natural propensity to evaporate ; the real fact is this, *the evaporation is not, and cannot be proportioned to the absolute surface (ceteris partibus) of liquids, but must be, and is, proportioned to the liberty of the surfaces themselves.* By the words *liberty* of the surfaces, I mean their not being counteracted by an electricity homologous to that by which they are themselves animated." The illustrious Beccaria then proceeds to the following description of experiments which he made on the same inquiry.

" I procured small cans of tin, two inches high, cylindrical, and eight lines wide. I filled one with water, and placed it at the bottom of the electric well : * another of the cans I left in the open air, and put into it only half an inch of water, and placed it six feet distant from the prime conductor. I placed next to it a third can, entirely filled with water ; afterwards I insulated both the cans and the electric well, and made them communicate with the conductor

* For a description of the electric well, see " Annals of Electricity, &c.," vol. vi. p. 415, plate v.

by iron wires that touched their bottoms. Now, neither the can placed at the bottom of the well, nor that in which the surface of the water was 18 lines lower than the brim of it, had, in three hours time, lost anything of their weight, but the can which was quite full had lost a grain, and even something more, of its weight.

“1. I suspended to the conductor one of the same cans, full of water, at the distance of three inches from the inferior surface of it, and to the bottom of this can I fastened a plate of tin, one foot wide. 2. I fixed another can on the conductor itself, which was also full. 3. I insulated two other cans, at six feet distance from the conductor; the one was on the one side of the conductor, and the other on the other side. Over the one I suspended, at five inches distance from its surface, a circular plate of tin, one foot diameter. The electricity having been continually excited for three hours together, I found that the can suspended to the conductor had lost nothing of its weight; that which was placed upon it had lost about half a grain; and that which had nothing suspended over it had lost about a grain; and the last, above which a plate of tin was suspended, had lost a grain and a half of its weight.

“Hence we see, that the evaporation was nothing where the surface of the water was counteracted on all parts by an homologous electricity. In the can placed on the conductor, only a little evaporation had taken place, because the atmosphere of the conductor bent itself over the surface of the water in that can, and lessened the evaporation. In the can placed at a great distance from the conductor, the evaporation was great, but it was still greater in the can over which the plate of tin was suspended, because a contrary electricity was actuated in that plate by the atmosphere of the can. In consequence of this, a continual dissipation of vapour was effected by invisible effluvia of the electric fire, which, through the plate were enabled to be diffused away. From the same fact we may also derive a new confirmation of the principle above mentioned, that the electric fire carries and disposes conducting (defferent) bodies in its way, &c.

“Thence we see, that *the evaporation of liquids* is not always *proportioned to the ampleness of their surfaces*; because, as the electric atmospheres of the liquids are much less counteracted by the reaction of the atmosphere of the adjacent parts, close to the sides of the vessels in which they are contained, than in other places, their evaporation must, of course, be proportionally greater.

“A number of other circumstances may also alter the quantity of electrical evaporation. 1. A square vessel will, as I have experienced, produce a greater evaporation than a cylindrical one, because the electric fire more easily dissipates through the angles of such vessels. 2. The liquor in a given vessel will evaporate the faster, as the height of its edge above the surface of the water will be in a smaller ratio to the width of it; this is because the electricity of the edge will then counteract less the electricity which promotes the

evaporation of the fluid. With regard to other circumstances, such as the temperature of the air, the dryness or dampness of the weather, &c., I think it would be needless here to expatiate upon them."

In the second edition of "*Cavallo's Treatise on Electricity*," page 42, we find that "electricity strongly communicated to insulated animal bodies, quickens their pulse and promotes their perspiration. If it is communicated to insulated fruits, *fluids, and, in general, to every kind of bodies that are in a state of evaporation, it also increaseth that evaporation, and in a greater or less degree as those bodies are more or less subject to evaporate themselves*, as the vessels that contain them are conductors or electrics, and as they have a greater or less surface exposed to the open air."

This is precisely the conclusion at which the Abbé Nollet arrived, with the exception of the extent of "surface exposed to the open air." At page 101, Cavallo states that "a considerable quantity of electricity exists in the atmosphere, and is certainly employed for some great purposes of nature." "Hitherto," again he says, "electricity has *not been found concerned in any fermentation, evaporation, or coagulation*, although the clouds, the rain, the hail, the snow, and the fogs, are almost always electrified."

Cavallo's Treatise was published in the year 1782, and although, as appears by the preceding paragraph, the author was not aware of the fact, yet Professor Volta, of Como, had previously discovered that electricity is produced by the evaporation of water, as will appear by the following extract from the "*Philosophical Transactions of the Royal Society*" for 1782. Volta says,

"I mentioned, that after various attempts I at last succeeded in obtaining undoubted signs of electricity from the simple evaporation of water, and from various chemical effervescences; but as this is a fact not less interesting than new, it seems proper to subjoin in this place a faithful account of the experiments made for that purpose. The first set of experiments were made at Paris, in company with M. Lavoisier and M. De la Place, two intelligent philosophers, and members of the Royal Academy of Sciences. After I had shewn them my experiments with my condenser, they, as well as myself, began to entertain hopes of succeeding in the experiments on the evaporation, &c. Accordingly, M. Lavoisier ordered a large condenser, with a marble plane, to be made. The first experiment I attempted with this instrument, in company with M. De la Place, proved unsuccessful; but the weather at that time was bad, the room was narrow and full of vapours, and the apparatus was not quite in good order. M. De la Place and M. Lavoisier repeated those experiments in the country, and then they were attended with success, which incited us to repeat and diversify the experiments, by which means the discovery was completed; having obtained unequivocal signs of electricity from the evaporation of water, from the simple combustion of coals, and from the effervescence of iron filings

in diluted vitriolic acid. This observation was made on the 13th of April of the present year (1782), and the experiments were performed in the following manner:—In an open garden a long metal plate was insulated, which, by means of a large iron wire, was made to communicate with the metal plate of the condenser, laid on the piece of marble, which was kept continually warm by some lighted coals set underneath. This done, some chafing dishes, containing burning charcoal, were placed on the large insulated plate. The combustion of the coals was helped by a gentle wind. Some minutes after, the iron wire, by which the large insulated plate was connected with the metal plate of the condenser, was taken off; then, the metal plate being removed from the marble by its insulating handle, and presented to Mr. Cavallo's electrometer, it made the balls of it diverge with negative electricity. The experiment was repeated by placing on a large insulated plate four vessels, containing iron-filings and water, instead of the chafing-dishes; then some vitriolic acid was poured into those four vessels, sufficient to cause a vigorous effervescence, and when the strongest ebullition was going to subside, the metal plate of the condenser was removed from over the marble, and being examined, not only electrified the electrometer with negative electricity, but gave a sensible spark. At this time, having tried to obtain electricity from the evaporation of water, the effects were equivocal or hardly sensible. The same thing happened a few days after, when, however, we obtained clear signs of electricity from those effervescences, which produce fixed and nitrous air. One day the electricity arising from the evaporation of water seemed to be positive; but subsequent experiments, and other circumstances, indicate that such a phenomenon must be attributed to a mistake.

“The experiment on the evaporation of water, which did not answer so well at Paris, succeeded much better in London, where I bethought me of throwing water on the lighted coals, which were kept in an insulated chafing-dish. In this manner the electricity of the evaporation never fails to electrify the chafing-dish *negatively*, and strongly enough for the electricity to be discovered by the simple electrometer: it will even afford a spark, if the condenser be used. Another time this experiment was repeated with success at Mr. Cavallo's, in the following manner:—A small crucible, containing three or four small coals lighted, was insulated; then a spoonful of water was thrown on the coals, and immediately after an electrometer, which communicated with the coals by means of a wire, diverged with negative electricity.

“The experiments hitherto made, though not numerous, yet concur to shew that the vapour of water, and in general the parts of all bodies that are separated by volatilization, carry away an additional quantity of the electric fluid, as well as elementary heat, and consequently that those bodies, from the contact of which the volatile particles have been separated, remain both cooled and elec-

trified negatively; from which it may be deduced, that whenever bodies are resolved into a volatile elastic fluid, their capacity for holding the electric fluid is augmented, as well as their capacity for holding common fire, or calorific fluid.* This is a striking analogy by which the science of electricity throws some light on the theory of heat, and alternately derives light from it: I mean on the doctrine of latent or specific heat, the first notions of which were suggested by the admirable experiments of Dr. Black and M. Wilkie, and which has been afterwards much elucidated by Dr. Crawford, who followed the experiments of Dr. Irwin. By following this analogy, it seems that as the vapours, on their condensing, lose part of their latent heat, on account of their capacity being diminished, so also they part with some electric fluid. Hence originates the positive electricity, which is always more or less predominant in the atmosphere when the sky is clear, viz., at the height at which the vapours begin to be condensed. Accordingly, the atmospherical electricity is stronger in fogs, in which case the vapours are more condensed, so as to be almost reduced into drops, and is still stronger when thick fogs become clouds.

“Hitherto we have accounted for the positive electricity, but it is easy to account for clouds negatively electrified; for when a cloud, positively electrified, has been once formed, its sphere of action is extended a great way round, so that if another cloud comes within that sphere, its electric fluid, agreeably to the well-known laws of electric atmospheres, must retire to the parts of it which are most remote from the first cloud; and from thence the electric fluid may be communicated to other clouds or vapours, or terrestrial prominences. Thus, a cloud may be electrified negatively, which cloud, after the same manner, may occasion a positive electricity in another cloud, &c. This explains not only the negative electricity, which is often obtained from the atmosphere in cloudy weather, and the frequent changes from positive to negative electricity, and contrarywise, in stormy weather; but also the waving motion often observed in clouds, and the hanging down of them so as nearly to touch the earth. After the forementioned discoveries, we need no longer wonder at the appearance of lightnings in the eruptions of volcanoes, as was particularly observed in the late dreadful eruption of Mount Vesuvius. The few experiments I have made, shew that the quantity of smoke, but much more the rapidity by which it is produced, tends to increase the electricity which arises from combustion, &c. How great, then, must be the quantity of electricity that is produced in such eruptions?”

It will be observed, by comparing the opinions of Dr. Franklin and M. Volta, respecting the electricity of the clouds, that they are very different to each other. Franklin considers the *general* electric

* This is precisely the view taken by Dr. Franklin, at least 33 years previously.—EDIT.

condition of the clouds to be *negative*, and that positively electric clouds are comparatively but of rare occurrence. Volta, on the other hand, considers the *general* electric condition of clouds to be *positive*, and accounts for negatively electric clouds as a secondary occurrence, occasioned by the repulsive influence of a more formidable electro-positive cloud. Hence Volta's opinion is, that *all clouds*, at their formation, are positively electric. Now, although the latter view is exactly the same as our own, it must be confessed that M. Volta has by no means shewn how it happens that vapour becomes positively electrical. That philosopher, according to the views of Dr. Franklin, considers that water, when converted into vapour, assumes a greater capacity for the electric fluid than before; and, therefore, would become *negatively* electrical the moment that it assumes the shape of vapour, conformable to Franklin's *can and chain* apparatus. That this is the fact we are quite certain, even though the vessel, with its remaining water, become *negatively* electric also. To clear up this point, we have only to consider that, as soon as the first stratum of steam or vapour is formed on the surface of the water, it has a greater capacity for the electric fluid than before, and consequently, for a short time, assumes a *negative* electric condition: but this new electric condition of the vapour allows of its being invaded by a portion of that electric-fluid which was left in the water, and also by a portion of that of the vicinal air; by which means it becomes supplied to a certain extent, and thus becomes *less negative* than at first, and in a short time afterwards it will have attained the whole quantity that is due to it as a vapour, but not more: therefore, it would be impossible for the vapour to assume a positive electric state by the simple process of evaporation, though it is quite certain that it may assume a *negative* electric state for a considerable time when its formation is very rapidly carried on. Why, then, did Volta's experiments shew that the cloud of vapour was positively electrical? Volta's experiments *did not* shew that the vapour was positively electrical. The indications of the electrometer, in those highly interesting experiments, were occasioned by the vapour *condensing*, and again becoming *water*, on the metallic appendages of his electroscope. The vapour, being condensed, had its capacity for the electric fluid *lessened*, and now yielded to the instrument a portion of the electric fluid which it had absorbed whilst in the state of vapour: hence the reason of the electroscope indicating positive electric action. It is easy to see how the dish and water would be left in a negative electric condition, which would also be the case with the vicinal air. It is thus, also, that aqueous vapours from the earth, by assuming a greater capacity for the electric fluid than they previously had whilst in the state of water, absorb, and carry up, an immense quantity of it; and that vapour must either be electro-negative or electro-neutral, until condensation again takes place, and can never become electro-positive until the *degree* of condensation

is sufficiently great to lower the electric capacity beyond that required to maintain an electric equilibrium with the surrounding air.

The curiosity of philosophers, concerning Volta's experiments, having soon become subsided, we hear very little more of the production of electricity by evaporation till the celebrated experiments of Messrs. Armstrong and Pattison, in the neighbourhood of Newcastle-upon-Tyne, the particulars of which have been made known to the readers of these Annals of Electricity, &c., vol. v., pp. 452, 453, 456. Vol. vi., pp. 37, 42, 305, 311.

The results of the *scientific* part of these famous experiments are perfectly reconcilable to the theoretical views we have already taken respecting the electrical condition of vapours in M. Volta's original experiments, viz., that vapour or steam, at its first production, is in a *negative* electrical condition, and that it is only by recondensation that it shews positive electric action.

Dr. Charles Schafhaentl made a series of experiments, from which he also concludes that the development of the positive electricity is occasioned by "the sudden condensation and separation of water from the steam."* But he also supposes that the steam itself, in order to produce electricity, should be produced in a peculiar manner, "by the sudden boiling of the water, and the conversion of a portion of it into a fine spray, &c." Such an inference might also be drawn from Volta's experiments, in which he obtained the greatest electric action by throwing a spoonful of water on burning coals. There can be no doubt of the action being *greater* under these circumstances; but we are far from considering that this condition is essential to the *absolute* production of electric action by evaporation. Indeed, our own experiments shew the contrary.—EDIT.

* Dr. Schafhaentl's experiments and reasoning thereon will shortly be placed before the readers of these Annals.

An Account of the Magnetic Observations made at the Observatory of Harvard University, Cambridge, by JOSEPH LOVERING, Hollis Professor of Mathematics and Natural Philosophy, and W. CRANCH BOND, Astronomical Observer to the College.

(*Memoirs of the American Academy.*)

(Concluded from page 112.)

In 1837, Gauss published his "*Allgemeine Theorie des Erdmagnetismus.*" This was the first attempt to subject the problem of the earth's magnetism to strict mathematical analysis. The solution was embarrassed and complicated, being of the nature of those which had already been performed in determining the figure of the earth and the tides. It required the use of Laplace's celebrated co-efficients, a powerful instrument, but difficult of management.

Besides, it laboured under the peculiar disadvantage of not being supplied with sufficient data, derived from observation, for calculating with precision the value of the constants. The whole developement may be found in the original or translated memoirs of Gauss, and a general idea of the analysis can be obtained from the able article on Terrestrial Magnetism, in the *London Quarterly Review*, to which reference has already been made. What is here called a theory, makes none or the most general assumptions as to the nature and distribution of magnetism in our planet. The investigation, which is mathematical throughout, depends, at last, on ascertaining the values of certain constants from observed data. Here the want was felt of a complete series of such as were nearly accurate and strictly comparable. It cannot be entirely relieved until the accomplishment of the present magnetic enterprise. With insufficient data, but the best that the state of science afforded, many of which were obtained through the assistance of the German and Russian Magnetic Association, Gauss calculates on the principle of least squares, which allows more places on the earth to be represented than there are unknown quantities, the values of his co-efficients. After passing the formulæ through several new forms, the chief object of which was to make them more simple and to facilitate the application, he brings the three components of the function into the following shape :—

X , Y , and Z , are the three co-ordinates of the magnetic force, exerted upon a given point of the earth, whose longitude is reckoned east from Greenwich. The auxiliary angles A^I , A^{II} , &c., B^I , B^{II} , &c., C^I , C^{II} , &c., depend upon the latitude.

$$X = a^{\circ} + a^I \cos. (\lambda + A^I) + a^{II} \cos. (2\lambda + A^{II}) + a^{III} \cos. (3\lambda + A^{III}) + a^{IV} \cos. (4\lambda + A^{IV}).$$

$$Y = b^I \cos. (\lambda + B^I) + b^{II} \cos. (2\lambda + B^{II}) + b^{III} \cos. (3\lambda + B^{III}) + b^{IV} \cos. (4\lambda + B^{IV}).$$

$$Z = c^{\circ} + c^I \cos. (\lambda + C^I) + c^{II} \cos. (2\lambda + C^{II}) + c^{III} \cos. (3\lambda + C^{III}) + c^{IV} \cos. (4\lambda + C^{IV}).$$

Professor Peirce has calculated the value of X , Y , and Z , by these formulæ for the Cambridge Observatory, whose longitude is $71^{\circ} 7'.5$ W., and whose latitude is $42^{\circ} 22'$ N. Here $\lambda = 288^{\circ} 52'.5$. The equations give these values for the co-efficients and auxiliary angles :—

$a^{\circ} = + 645.9$	$b^{\circ} = 0$	$c^{\circ} = + 1300$
$\log. a^I = 2.29185$	$\log. b^I = 2.23230$	$\log. c^I = 2.36763$
$\log. a^{II} = 1.74467$	$\log. b^{II} = 2.04629$	$\log. c^{II} = 2.19893$
$\log. a^{III} = 1.36560$	$\log. b^{III} = 1.59317$	$\log. c^{III} = 1.62487$
$\log. a^{IV} = 0.75277$	$\log. b^{IV} = 0.92425$	$\log. c^{IV} = 0.88968$
$A^I = 245^{\circ} 55'$	$B^I = 353^{\circ} 17'$	$C^I = 90^{\circ} 34'$
$A^{II} = 278 \ 42$	$B^{II} = 56 \ 55$	$C^{II} = 157 \ 13$
$A^{III} = 243 \ 31$	$B^{III} = 322 \ 2.5$	$C^{III} = 46 \ 19$
$A^{IV} = 142 \ 26$	$B^{IV} = 232 \ 26$	$C^{IV} = 322 \ 26$

then we have :

$$\begin{array}{l|l|l} \lambda + A^I = 534^{\circ} 47'.5 & \lambda + B^I = 642^{\circ} 9'.5 & \lambda + C^I = 379^{\circ} 26'.5 \\ 2\lambda + A^{II} = 856 \quad 27 & 2\lambda + B^{II} = 634 \quad 30 & 2\lambda + C^{II} = 734 \quad 58 \\ 3\lambda + A^{III} = 1110 \quad 8.5 & 3\lambda + B^{III} = 1188 \quad 40 & 3\lambda + C^{III} = 912 \quad 56 \quad .5 \\ 4\lambda + A^{IV} = 1297 \quad 56 & 4\lambda + B^{IV} = 1387 \quad 56 & 4\lambda + C^{IV} = 1477 \quad 56 \end{array}$$

and consequently :

$$\begin{array}{l} + 645.9 = a^{\circ} \\ \log. a^I + \log. \cos. (\lambda + A^I) = 2.29006 = - 195.01 = a^I \cos. (\lambda + A^I) \\ \log. a^{II} + \log. \cos. (2\lambda + A^{II}) = 1.60487 = - 40.26 = a^{II} \cos. (2\lambda + A^{II}) \\ \log. a^{III} + \log. \cos. (3\lambda + A^{III}) = 1.30251 = + 20.07 = a^{III} \cos. (3\lambda + A^{III}) \\ \log. a^{IV} + \log. \cos. (4\lambda + A^{IV}) = .64970 = - 4.46 = a^{IV} \cos. (4\lambda + A^{IV}) \end{array}$$

$$X = 426.20$$

$$\begin{array}{l} \log. b^I + \log. \cos. (\lambda + B^I) = 1.55579 = + 35.96 = b^I \cos. (\lambda + B^I) \\ \log. b^{II} + \log. \cos. (2\lambda + B^{II}) = .95669 = + 9.05 = b^{II} \cos. (2\lambda + B^{II}) \\ \log. b^{III} + \log. \cos. (3\lambda + B^{III}) = 1.09840 = - 12.54 = b^{III} \cos. (3\lambda + B^{III}) \\ \log. b^{IV} + \log. \cos. (4\lambda + B^{IV}) = .71294 = + 5.16 = b^{IV} \cos. (4\lambda + B^{IV}) \end{array}$$

$$Y = 37.6$$

$$+ 300.00 = c^{\circ}$$

$$\begin{array}{l} \log. c^I + \log. \cos. (\lambda + C^I) = 2.34214 = + 219.85 = c^I \cos. (\lambda + C^I) \\ \log. c^{II} + \log. \cos. (2\lambda + C^{II}) = 2.18394 = + 152.74 = c^{II} \cos. (2\lambda + C^{II}) \\ \log. c^{III} + \log. \cos. (3\lambda + C^{III}) = 1.61370 = - 41.09 = c^{III} \cos. (3\lambda + C^{III}) \\ \log. c^{IV} + \log. \cos. (4\lambda + C^{IV}) = .78661 = + 6.12 = c^{IV} \cos. (4\lambda + C^{IV}) \end{array}$$

$$Z = 1637.6$$

X , Y , and Z , being thus determined, if we represent the declination by δ , the inclination by i , the total intensity by ψ , and the horizontal intensity by w , we have these two formulæ to find δ and w :—

$$X = w \cos. \delta, \quad Y = w \sin. \delta.$$

Again ;

$$w = \psi \cos. i, \quad Z = \psi \sin. i.$$

from which i and ψ are deduced.

$$\begin{array}{l} \log. Y = 1.57519 \\ \log. X = 2.62961 \end{array} \quad \log. X = 2.62961$$

$$\frac{Y}{X} = \tan. \delta = 8.94558 \text{ sec. } \delta = 0.00170$$

$$\begin{array}{l} \delta = 5^{\circ} 4 \quad 2.63131 = X \text{ sec. } \delta = w = 427.9 \\ \log. Z = 3.21421 \quad \log. Z = 3.21421 \end{array}$$

$$\log. \text{cosec. } i = 0.01433 \quad \tan. i = 0.58290 = - \frac{Z}{w}$$

$$\psi = 1692.5 = 3.22854 = Z \text{ cosec. } i \quad i = 75^{\circ} 22'$$

After dividing the numbers which represent the horizontal and total intensity by 1000 to reduce them to the arbitrary unit in com-

mon use, we have the following values of the elements for 1837, calculated according to Gauss' formulæ :—

	Computed.	Observed.	Difference.
The Declination	= 5° 4'	9° 9'	4° 5'
The Dip	= 75 22	74 22	1 00
The Horizontal Intensity	= 0.4279		
The Total Intensity	= 1,6925		

We are now able to add one more to the list of 99 places for which Gauss has compared the computed and observed values of the elements. The difference which appears in all cases between the two is produced by several causes. The observations are not contemporaneous, and they are visited by accidental errors and the strange anomalies of the magnetic force. The co-efficients, which depend upon the grouping of the observed values, must suffer from the same influences; and hence the computed places, by no fault of the theory, are involved in uncertainty. In some cases the two errors may balance each other; at other times they will conspire to produce a great difference. Thus we explain those considerable discrepancies which occasionally appear between the results of observation and calculation. So far as declination is concerned, Cambridge suffers particularly from these causes; there are only 3 out of the 97 places for which Gauss has made the computation where the difference is so great between the computed and observed element. In these instances, it amounts respectively to 5° 45', 4° 42', and 5° 15'. In regard to inclination the case is more favourable, as there are 40 places in Gauss' catalogue where the difference is greater than at Cambridge; the maximum difference being 4° 38', or 5 times that of the latter place. Out of the 98 places for which the declination has now been calculated, the difference is plus in 52 instances, and minus in 46; and out of 100, for which the inclination has been computed, the difference is plus 66 times, and minus 34 times. This is satisfactory proof that the error proceeds from the observations, and not from the theory. Before sentence can be fairly pronounced upon the latter, better observations must be possessed for comparison and the determination of the arbitrary co-efficients. It is especially to be desired that contemporaneous observations of great accuracy should be made in every quarter of the globe, that the calculated values of the co-efficients may be general and impartial. Until this is done we cannot expect that the theory, however complete in itself, will give correct expressions of the elements for all parts of the earth. Thus we explain the large difference between the calculated and observed declination for Cambridge and a few other places. We must, however, never lose sight of the fact, that remarkable local disturbances may sometimes derange the observed value of the element, so as to leave still a few cases of unusual discrepancy. Part VII. of the "Scientific Memoirs" contains maps of the lines of declination and inclination on the globe for 1838, drawn by Gauss according to his theory. In

Silliman's American Journal, No. 2 of 1838, is a chart of Professor Loomis, which presents both these classes of lines as they crossed the United States in 1840; they were projected from a collection of all the observations that had been made in the country, after they were reduced to the same time. A comparison of the observed and empirical lines exhibit sufficient agreement to satisfy us of the general correctness of Gauss' theory in its application to this western continent. Mr. Loomis remarks, in a paper published in the Philosophical Transactions of Philadelphia,* that the same dip is found in a higher latitude in the western than in the eastern states. By recurring to Gauss' map of the lines of inclination, we notice the same singularity in the empirical lines. So also on Gauss' map of declination, the lines which are far apart in Europe are convergent in the northern states as they approach the magnetic pole. We can readily conceive, therefore, that an error in the value of the coefficients, which should hardly be felt in one place, may be magnified into great importance at the other. We dismiss this interesting discussion with the confident hope that the simultaneous and extensive observations of the magnetic elements, which are now in progress, will soon relieve the author of the theory from the embarrassments to which the want of accurate data subjected him; and enable him to predict with certainty, concerning the magnetic state of any part of the globe, though it has never been approached by man.

We shall close this communication with a brief description of the new magnetic observatory at Cambridge. The buildings which have been erected for this purpose are represented on the right hand of plate I; they connect with the observer's house at the north-west corner, through a covered passage. Their shape, as seen on the ground plan, was determined by the positions required for instruments which they were to contain. The impossibility of ascertaining the dip with accuracy, has caused it to be dropped as one of the primitive elements of terrestrial magnetism, and the horizontal and vertical forces have been substituted in its place. The three instruments in use at the magnetic observatories established by the Royal Society, and recommended to all the allies, are the declination magnetometer, and the horizontal and vertical force magnetometers. The first two are inventions of Professor Lloyd, and correspond in their objects and part of their construction to the Gauss declination magnetometer and his bifilar magnetometer. Instead of the mirror, and the scale to be reflected from it, these bars are furnished with two sliding pieces, one at each end. That farthest from the observer contains a finely divided scale of glass; the other an achromatic lens. The distances of the two are so arranged that the scale shall be in the focus of the lens. It is evident that the apparatus thus adjusted forms a moving collimator. The scale is carried by the bar in all its changes of position, and, being read off

* Transactions of the American Philosophical Society. Vol. VII., New Series. Part I. Philadelphia. 1840.

by a fixed telescope at a suitable distance, records the variations of magnetic declination. It is extremely easy to pass from the readings of the scale to absolute values. It is only necessary to find the azimuth of the line of collimation of the telescope by a variation-transit, and then adding or subtracting the difference between the former and a fixed point of the scale, determined by observation, we readily convert every observation into absolute declination. In this respect the instrument of Lloyd is preferable to the Gauss magnetometer, where the passage from marks on the scale to their corresponding absolute values is difficult and uncertain. A more minute description of Lloyd's apparatus may be found in the report of the Royal Society, to which we have made frequent reference. It was first proposed that the place of the bar should be marked at three successive excursions, and the position determined by the familiar formula $\frac{1}{2}(a + 2b + c)$; where a , b , and c express the three readings. But a circular was issued to the magnetic observatories, under date of January 15, 1841, and signed by Professor Lloyd, recommending that the method of observing the declination and horizontal intensity magnetometers be so far modified, as that, instead of the three successive readings just mentioned, they should be taken at three times $T-t$, T , and $T+t$, of which T is the appointed epoch of observation, and t the mean time of vibration of the magnet. This is returning essentially to the principle of Gauss, which gives the position of the bar for the precise moment required, and which we should think to be generally superior, whenever the length of the arc of vibration admits of it, to the method of observing three successive excursions. To recur now to the plate; b is the place of the declination magnetometer; g and g windows to allow the light to pass in so as to be reflected strongly on the scale; e is the place of the fixed telescope; a is a pillar which supports the variation-transit. This instrument can be adjusted in the meridian by independent observations of the stars, through a section in the roof at h , or more easily by inverting the transit at B , and using it as a collimator. If the variation-transit be directed to the mid-wire of the large transit, so that while its line of collimation falls behind the latter, it also intersects the mark on Blue Hill; then we know that it is in the same meridian line as the transit, and by turning it over to the north, and reading off the azimuth of the line ae , we find the variation of the fixed telescope, and consequently, the absolute value of every reading that is made with it. We pass now to the horizontal force magnetometer, which differs from the bifilar magnetometer of Gauss only in employing a moveable collimator instead of the mirror. The principle of either instrument is easily explained. A magnetised bar is suspended by a double wire, attached to two points near its centre at a definite distance apart, which remains the same through their whole length. The cap at the top, from which the suspension is made, is then turned until the torsion suffices to bring the bar round at right angles to the magnetic meridian.

The horizontal component of the magnetic force of course tends to bring it back to the magnetic meridian, and therefore its mean value is measured by the mean torsion. But as the torsion remains nearly constant at the same angle, any change in the horizontal force is indicated by the motion of the bar to the one side or the other of the perpendicular direction. This small variation from a mean position is observed in the same way precisely as the variations of declination in the first instrument; the change of horizontal intensity is a simple function of this small arc, and very easily determined. When the absolute horizontal intensity is required, it is found by the method which Gauss employs with his bifilar magnetometer; c is the place of the horizontal force instrument; f is the place of the fixed telescope, whose position at right angles to the magnetic meridian is readily adjusted by the variation-transit at a . Hence, all the readings of this instrument can be readily converted into absolute determinations by applying the variation with the proper sign, as a correction to the mean absolute value. Again, d represents the position of the vertical force magnetometer. It consists of a magnetized bar resting by knife-edges on agate planes, and loaded so as to be horizontal at the average value of the vertical force. It is evident that any change in the amount of this force will be indicated by a motion of the arms of the lever one way or another. Any motion of this sort is observed by fixed microscopes with micrometer wires in front of each end of the bar, the double reading securing greater accuracy. Two cross wires in a hollow circle on each extremity of the bar determine for the eye its axis; and the changes of the vertical force are a function of the variations of this axis from its mean horizontal position. Both the horizontal and vertical force instruments are furnished with thermometers, and corrections for temperature are applied in the reduction of the observations to a definite unit. Thus we have the means of observing the three approved elements of the earth's magnetism. In regard to the vertical force, however, no way that is not open to practical objections has been devised of obtaining its absolute value. It is recommended, therefore, to obtain it indirectly from the formula $V = H \tan i$, where H represents the horizontal intensity and i the inclination. Thus, we are still subject to the inconvenience of observing the dip of the needle with the common dipping apparatus for absolute determinations of itself and the vertical force. The last circular to the magnetic observatories advises that observations be made on the dip *Tuesdays in the forenoon and Fridays in the afternoon*. Various modes of observing this instrument are in use, and the best result is probably obtained by a combination of all of them.

The buildings which contain these instruments are made of wood with copper nails. Iron has been carefully excluded from every part. In order that the corrections for temperature may be small,

they are furnished with a soapstone stove having a copper funnel.* The instruments rest upon blocks of red sandstone, which are firmly set on the ground, insulated from the vibrations of the floor, and known to be free from magnetic influence. It is of the first importance to secure the several magnetometers from mutual interference. They are all too distant from the Gauss magnetometer at *C* to give or receive any disturbance in that quarter. The mutual action of two magnets diminishes as the cube of the distance, and greater security has been placed in this mode of reducing it than more complex ones. Thus the distance from *b* to *c* is 36 feet 9 inches; from *b* to *d* is 36 feet 2 inches; from *c* to *d* 40 feet 6 inches. The distances of the fixed telescopes *e* and *f* from *b* and *c* respectively are 7 feet; from *a* to *b* it is thirty feet, and the width of the building is 6 feet. Biot, Gauss, and Lloyd, have investigated the problem of the mutual action of a system of magnets; and positions have been found for a limited number of bars in which the disturbance should be nothing or constant; in the latter case, a simple correction applied to all the observations, or which might be made at once in the construction of the scale, if the arrangement were previously known, is all that is necessary. Out of several combinations of this kind for three magnets, which the general theory discloses, we have selected the one recommended by Weber. The line *a b*, of course, is the direction of the magnetic meridian; *a c* is perpendicular to it; then the point *c* is so selected that the angle which the line *b c* makes with the magnetic meridian at *b* may be equal to 35° (theoretically it should be $35^\circ 15' 52''$). Now, we are sure that the mean place of the declination instrument is not affected by the disturbance of *c* upon *b*, since it acts in the direction of the magnetic meridian. But the deviations from it will be affected by a constant error for which a correction must be applied. Again, the horizontal force instrument is practically affected only in regard to the divisions of the scale. The tangential part of the disturbance tends to move the needle from its transverse position; but this is prevented by the suspension, and enters unperceived into the calculation of the absolute value of the element. Again, the angle which the line *b d* makes with the magnetic meridian is also 35° ; so that *b* is affected by *d* in the same manner as by *c*. The action which *b* would exert upon *d*, being in a horizontal direction, can have no influence in deranging *d*, which admits only of motion in a vertical direction.

These three instruments have been in adjustment two months, and regular observations were made with them on the term-days of March and April, 1841; and will hereafter be continued. The same observer, by constant attention, is able to tend the three instruments at once. Thus, if the time were 10 P. M. Gott. M. T.,

* In the Magnetic Observatory at Munich, the instruments are placed in a room 13 feet below the surface of the ground, where the temperature varies but little through the year.

b would first be observed at 0^m; then *c* at 2^m 30^s, then *b* again at 5^m, then *d* at 7^m 30^s, then *b* a third time at 10^m, and *c* a second time at 12^m 30^s, *b* a fourth time at 15^m, and *d* a second time at 17^m 30^s. In this way, which is continued during the whole period of observation, we have one observation of the declination every 5 minutes, and one of the horizontal and vertical forces every 10 minutes. On the March term-day, which was the time when the transition was made from the Gauss to Lloyd's magnetometer, both declination instruments were observed, in order to see how far they were comparable. At first sight the two curves, with a general agreement, exhibited some alarming discrepancies; but these have been referred to the stretching of the suspension threads of the new instrument, and are not likely to recur again. Another opportunity will be taken of comparing the two instruments together after that of Lloyd has attained its proper bearings. Fig. 2 of Plate II. represents one hour's observation with these declination magnetometers. As the apparatus for all the magnetic observatories is from the hands of the same artist, the observations will be eminently comparable. A few years of such observations, conducted on a systematic and uniform plan, will avail more in solving the intricate problem of terrestrial magnetism, and reducing it to a theory, than the whole mass of scattered and disorderly data that have accumulated since the days of Gilbert.

TABLE I.

Gottingen Time, reckoned from Mean Noon.	1840. March. 27-28.	May. 29-30.	June. 24-25.	July. 24-25.	Aug. 28-29.	Sept. 23-24.	Oct. 21-22.	Nov. 27-28.	Dec. 23-24.	1841. Jan. 20-21.	Feb. 26-27.	March. 24-25.
X— 0	106.9	109.3	104.1	100.3	116.5	111.0	96.0	96.7	91.1	91.0	93.1	92.7
5	107.1	109.4		100.2	116.5	111.4	96.0	96.8	90.3	90.6	92.7	95.1
10	107.3	107.1		100.1	116.2	111.4	96.2	96.5	90.1	91.3	92.0	94.1
15	107.5	106.8		99.9	115.0	110.8	96.4	96.2	89.8	92.9	91.2	92.3
20	107.9	108.9	103.8	100.1	116.4	111.8	96.4	95.8	88.9	94.9	91.1	91.4
25	108.3	108.9		100.2	117.3	112.5	95.9	95.7	89.1	95.2	92.0	92.4
30	108.9	111.6		100.1	122.4	114.6	96.1	95.9	89.2	94.5	91.3	91.6
35	109.6	106.5		100.2	126.9	116.3	98.8	96.4	89.7	94.3	92.0	90.8
40	110.2	109.2	103.7	100.3	134.1	117.9	101.7	96.2	90.0	94.1	92.3	91.5
45	110.4	111.7		100.5	134.9	117.5	104.5	96.0	90.5	93.6	93.3	92.2
50	110.7	112.0		100.6	131.7	114.7	102.1	97.1	90.1	93.2	93.3	94.2
55	110.7	113.5		100.7	129.7	112.4	100.5	97.2	90.3	93.2	92.8	94.0
XI— 0	110.5	111.6	103.2	100.7	127.4	110.6	99.0	98.3	91.1	93.3	91.8	93.3
5	110.3	106.0		100.7	125.1	109.0	97.1	98.8	91.9	93.4	90.2	92.4
10	110.6	110.1		100.8	123.4	106.1	98.5	99.6	93.6	93.4	89.8	94.3
15	111.0	111.3		100.4	122.4	104.8	99.7	99.4	95.0	93.7	90.0	93.7
20	110.9	106.0	103.8	100.2	119.7	103.9	100.7	99.2	94.7	93.6	90.2	91.9
25	110.9	123.4		99.9	117.0	103.2	99.7	99.3	94.7	93.3	89.5	91.7
30	110.9	101.3		99.8	114.9	102.5	99.1	99.4	91.8	93.5	89.4	92.7
35	111.1	117.5		100.2	114.9	100.8	99.6	100.4	92.1	93.3	89.4	93.1
40	111.1	116.7	103.9	100.8	116.0	99.3	99.2	101.2	93.2	93.5	89.4	92.1
45	110.7	109.4		100.9	117.5	98.6	99.1	101.5	95.2	93.7	90.4	94.2
50	110.8	107.7		101.0	120.2	98.4	97.7	101.6	94.0	93.9	91.3	96.3
55	110.9	112.1		101.0	118.1	98.1	97.9	101.3	93.3	94.0	91.7	96.7
XII— 0	111.4	113.3	103.4	100.8	116.9	98.4	98.2	100.1	92.0	94.1	91.8	98.1
5	111.7	117.1		100.6	115.7	98.5	99.7	99.8	92.6	94.3	91.3	97.6
10	111.6	116.5		101.0	114.5	99.1	99.7	99.7	93.4	94.7	90.6	94.9
15	111.2	113.5		100.8	112.6	98.7	100.5	99.7	93.5	94.9	90.7	92.5
20	111.1	109.6	103.7	100.8	110.5	98.7	101.5	99.7	94.0	95.1	91.7	91.7
25	111.0	106.7		100.7	110.3	98.7	101.9	100.4	94.7	95.2	92.5	92.6
30	111.0	105.6		100.5	111.1	99.4	101.7	101.0	93.5	95.3	92.0	98.6
35	111.1	108.1		100.4	112.2	98.9	101.9	101.3	92.9	95.2	91.2	107.5
40	111.2	110.3	103.7	105.3	112.9	98.8	104.2	101.6	93.0	95.1	89.3	107.4
45	111.5	110.7		101.7	113.6	98.8	107.3	100.8	93.6	95.0	91.6	108.0
50	111.8	110.6		102.4	114.1	98.7	108.4	101.6	94.4	94.8	95.3	105.9
55	112.1	111.1		103.4	113.5	98.8	108.5	100.9	96.6	94.7	104.8	102.8
XIII— 0	112.4	113.1	103.2	103.3	114.5	99.5	106.6	101.5	102.3	94.8	103.6	104.8
5	112.6	113.5		102.4	114.1	99.1	106.1	101.6	103.4	95.0	104.1	104.6
10	112.7	113.4		101.2	114.2	100.6	106.4	101.7	103.9	95.4	104.5	103.5
15	112.9	116.5		99.9	115.1	102.2	105.4	101.8	102.5	95.7	105.6	101.1
20	112.8	115.7	103.5	99.2	113.8	103.5	105.5	102.2	101.5	95.9	104.1	101.4
25	112.5	122.3		99.0	111.9	105.3	105.2	97.2	99.9	96.5	103.2	101.2
30	112.4	122.6		98.6	113.3	104.9	104.4	101.8	102.3	96.7	101.3	102.0
35	113.0	122.7		98.7	115.6	105.5	104.0	102.2	103.9	96.8	98.8	101.8
40	113.5	124.3	104.0	98.9	117.0	109.6	103.3	101.2	102.3	96.9	96.1	101.4
45	113.8	123.1		98.7	116.8	109.6	102.9	101.1	99.5	97.2	96.3	100.5
50	113.6	118.4		98.7	116.7	109.3	102.7	100.2	100.2	97.2	96.4	99.5
55	112.8	121.8		98.3	117.1	107.1	101.7	100.6	100.9	96.9	97.6	99.7

TABLE I.—CONTINUED.

Gottingen Time, reckoned from Mean Noon.	1840. March. 27-28.	May. 29-30.	June. 24-25.	July. 24-25.	Aug. 28-29.	Sept. 23-24.	Oct. 21-22.	Nov. 27-28.	Dec. 23-24.	1841. Jan. 20-21.	Feb. 26-27.	March. 24-25.
XIV— 0	112.5	122.1	103.2	98.8	118.5	105.2	100.9	100.7	97.5	95.8	97.8	100.0
5		123.2		99.1	121.0	105.2	101.1	101.8	93.2	95.6	97.7	101.5
10	113.0	123.9		99.2	123.5	106.0	101.6	100.7	92.5	95.9	97.6	102.7
15	112.9	124.1		99.3	129.8	105.0	101.8	100.9	90.1	96.0	97.4	103.0
20	112.5	122.8	102.1	99.1	143.1	104.1	101.1	100.7	94.0	96.1	98.3	102.6
25	112.6	118.4		99.5	153.7	105.3	100.6	101.2	95.7	95.9	98.3	101.0
30	113.3	113.5		99.6	162.3	105.7	100.9	101.0	95.4	95.9	98.0	100.5
35	114.2	113.9		99.7	164.4	104.2	101.7	100.9	94.5	96.1	98.1	100.3
40	114.9	116.7	101.0	99.6	164.0	103.4	102.3	100.7	96.8	96.4	97.9	100.0
45	115.7	114.6		99.6	153.9	103.0	103.2	100.5	98.7	96.4	98.5	99.5
50	116.3	111.5		99.8	145.1	102.2	103.6	101.7	100.3	96.7	98.3	100.4
55	116.6			100.0	142.6	101.6	101.6	100.6	100.7	96.7	98.4	99.9
XV— 0	117.5	108.7	101.9	100.0	140.3	102.2	101.9	101.3	98.3	97.1	100.0	100.7
5	118.7	113.6		99.8	138.0	102.7	99.2	101.5	99.2	98.2	101.7	100.6
10	118.6	115.6			143.9	101.7	98.4	101.2	98.2	100.0	103.2	99.9
15	119.0	118.6			139.9	100.8	98.4	101.6	101.3	102.0	102.0	99.1
20	117.5	116.0	101.9		137.0	100.7	100.0	101.8	103.9	103.2	101.6	98.8
25	117.1	117.1		100.6	130.9	100.8	102.4	101.7	103.6	103.5	95.9	98.0
30	116.3	113.4			124.8	101.2	103.5	99.7	103.0	102.5	93.4	98.9
35	115.4	110.0			111.5	100.3	103.3	100.9	101.6	101.1	94.4	100.0
40	115.0	101.8	103.9	101.7	120.4	97.9	102.7	100.5	100.6	99.3	99.4	102.1
45	114.1	96.3			127.7	97.4	102.2	99.8	102.0	97.7	99.7	102.7
50	113.9	101.7			129.8	98.3	101.9	100.9	102.5	97.9	100.9	101.6
55	113.8	114.4			129.2	100.0	106.1	100.9	100.7	97.1	106.9	100.1
XVI— 0	113.7	96.7	105.1	102.0	125.8	101.5	107.3	100.5	100.0	97.5	106.7	97.4
5	113.0	71.2			127.1	103.6	105.3	99.9	99.3	97.1	105.9	97.1
10	112.4	70.2			127.8	102.2	103.4	99.9	98.6	96.8	101.7	96.9
15	113.1	75.9			113.8	100.5	99.9	100.5	97.8	95.7	100.3	97.3
20	112.7	77.6	105.2	101.8	110.9	100.2	92.6	101.4	98.0	95.2	99.7	99.7
25	112.0	90.7			116.9	101.0	88.2	100.3	98.5	94.9	100.5	98.7
30	111.7	95.9			120.9	101.4	88.0	100.8	97.8	94.8	102.1	98.7
35	111.7	100.4			115.4	100.5	93.9	100.9	97.4	94.9	98.7	98.3
40	111.9	95.4	108.1	102.4	113.8	100.1	96.4	99.7	97.3	95.0	96.7	95.5
45	112.3	80.8			115.7	100.2	98.4	100.1	97.1	95.2	96.0	94.7
50	112.3	98.1			119.4	101.1	98.1	100.4	97.1	95.3	97.5	94.7
55	112.2	103.9			124.7	101.4	99.0	99.7	97.4	95.2	96.7	95.5
XVII— 0	112.0	97.7	106.2	102.2	129.1	103.3	98.7	100.2	97.6	95.0	97.0	94.7
5	111.9	95.7	106.8		125.5	103.9	97.7	99.9	98.5	94.8	97.3	93.7
10	112.1	112.4	107.5		119.6	103.9	99.7	99.8	99.2	95.0	95.9	93.7
15	112.2	121.2	108.0		112.7	104.1	102.9	99.1	99.6	95.9	97.0	92.7
20	112.2	116.6	108.2	102.5	108.5	103.8	104.9	99.3	99.6	95.3	96.2	93.7
25	112.5	106.0	108.2		111.4	102.9	105.0	99.2	99.4	95.3	96.6	93.7
30	111.6	104.9	108.2		115.3	102.1	104.2	99.3	99.2	95.1	96.2	94.7
35	109.6	116.4	108.0		156.1	102.8	103.1	99.3	98.8	95.0	96.7	94.7
40	107.8	191.4	107.7	101.9	137.5	103.8	102.5	99.3	98.9	95.0	96.0	96.7
45	107.7	117.3	107.5		137.0	104.4	102.3	99.4	98.7	95.0	97.1	96.7
50	107.2	119.4	107.3		136.2	104.0	102.6	99.1	98.6	95.0	96.6	96.7
55	108.1	119.1	107.2		118.2	103.2	102.1	98.9	98.7	95.2	95.8	97.7

TABLE I.—CONTINUED.

Longitude Time reckoned from Mean Noon.	1840. March. 27-28.	May. 29-30.	June. 24-25.	July. 24-25.	Aug. 28-29.	Sep. 23-24.	Oct. 21-22.	Nov. 27-28.	Dec. 23-24.	1841. Jan. 20-21.	Feb. 26-27.	March. 24-25.
XVIII— 0	109.3	123.4	107.3	101.4	122.0	103.2	101.4	98.6	98.5	95.2	95.7	95.4
5	110.3	121.9			140.7	102.3	101.2	99.1	99.3	95.2	94.1	95.2
10	111.1	119.2			138.5	101.5	100.9	98.6	99.3	95.2	94.3	95.2
15	111.7	118.2			128.7	100.8	101.1	98.3	99.7	95.2	95.4	93.1
20	112.5	121.9	107.5	102.0	121.0	101.2	101.0	99.4	99.5	95.4	96.1	91.3
25	112.3	118.2			122.5	101.9	100.9	99.4	97.3	95.4	95.6	89.7
30	111.6	114.9			128.6	102.7	100.9	99.8	93.7	95.2	95.7	91.1
35	111.1	111.3			126.2	101.6	101.6	98.9	92.7	95.2	95.9	91.7
40	110.4	105.9	107.4	101.3	108.0	99.6	101.6	99.7	91.7	95.3	96.0	92.6
45	111.6	101.0			103.7	99.5	100.3	99.5	89.7	95.2	96.6	93.2
50	111.3	96.0			108.0	100.1	101.2	98.9	89.3	95.0	95.7	92.3
55	111.4	94.4			114.0	101.4	101.8	99.0	90.7	95.4	95.1	92.1
XIX— 0	111.3	91.4	107.9	101.6	117.5	102.0	100.2	99.1	93.2	95.5	95.0	93.1
5	110.7	87.9			127.2	101.0	100.4	99.2	95.4	95.6	96.1	92.7
10	110.5	99.9			146.5	100.5	100.8	98.7	96.4	95.9	95.3	92.6
15	111.0	95.7			141.5	100.5	101.1	98.6	96.4	95.9	92.7	93.6
20	110.7	100.1	108.0	101.8	135.5	100.4	101.1	98.7	97.2	96.1	91.8	92.5
25	111.3	102.2			132.7	99.6	101.5	98.6	97.2	96.2	91.6	93.5
30	111.5	104.7			131.2	99.9	102.2	98.8	96.6	96.1	92.1	94.6
35	112.2	110.1			128.5	101.0	103.0	99.1	97.0	96.1	92.5	93.3
40	113.3	111.8	108.3	101.7	118.0	101.0	103.3	99.3	96.3	96.2	92.6	94.1
45	112.9	115.3			121.1	101.5	103.7	99.0	96.8	96.5	91.8	92.5
50	112.9	117.1			123.5	101.5	103.3	99.4	97.0	96.4	93.2	93.7
55	113.2	117.5			121.6	102.7	103.5	99.4	97.4	96.4	93.2	93.8
XX— 0	113.4	117.4	108.3	101.8	117.1	103.5	103.2	98.9	97.3	96.4	94.5	93.9
5	113.4	117.1			117.9	103.7	101.3	99.7	97.5	96.1	94.7	92.8
10	113.9	116.7			122.5	103.5	100.9	99.6	97.1	96.3	94.5	97.3
15	113.8	116.1			123.0	104.5	100.5	99.0	96.6	96.3	95.2	92.3
20	113.5	118.5	108.5	102.2	123.0	104.9	98.2	100.7	97.0	96.6	94.9	93.5
25	113.6	118.6			123.6	105.2	97.1	99.5	97.8	96.5	95.1	93.1
30	113.5	121.9			123.7	105.3	95.8	99.6	97.6	96.6	94.6	92.5
35	113.2	120.9			126.4	105.1	95.7	99.4	97.1	96.6	95.2	92.2
40	113.1	118.8	108.6	101.7	126.9	105.1	94.9	98.8	97.3	96.6	95.0	94.3
45	113.0	118.8			127.9	105.4	94.3	98.9	98.3	96.1	95.2	94.3
50	113.1	117.7			129.6	106.1	93.3	99.1	97.8	96.0	95.3	93.8
55	113.0	116.1			128.5	105.9	92.4	99.1	97.3	95.8	95.7	94.3
XXI— 0	113.0	114.7	108.6	101.7	127.0	105.9	91.7	99.5	97.2	95.8	94.9	93.5
5	113.0	114.5			129.2	105.1	92.8	99.7	97.1	96.0	94.1	93.5
10	113.0	114.1			131.7	104.0	94.3	99.6	96.8	95.9	94.1	92.9
15	113.2	113.9			129.4	103.0	96.0	99.8	97.0	95.9	94.4	91.2
20	113.7	113.9	109.0	102.1	130.7	102.2	96.6	100.5	97.6	95.7	92.7	90.0
25	114.0	113.5			130.4	101.0	98.4	99.3	97.8	95.6	92.2	89.1
30	114.2	114.4			130.2	100.3	98.9	100.4	97.5	95.9	92.0	89.5
35	114.3	113.8			131.7	100.2	99.4	101.0	97.9	96.0	92.9	89.2
40	114.0	113.5	109.2	102.9	131.0	99.6	100.1	99.6	97.9	96.0	93.0	88.6
45	113.9	113.4			131.3	99.4	101.7	100.3	97.8	96.1	93.9	89.0
50	113.7	112.5			128.9	97.9	101.2	100.5	97.7	96.3	94.4	91.1
55	113.4	111.7			128.5	97.1	101.1	100.2	97.7	96.1	94.2	90.8

TABLE I.—CONTINUED.

Gottingen Time, reckoned from Mean Noon.	1840. March. 27-28.	May. 29-30.	June. 24-25.	July. 24-25.	Aug. 28-29.	Sept. 23-24.	Oct. 21-22.	Nov. 27-28.	Dec. 23-24.	1841. Jan. 20-21.	Feb. 26-27.	March. 24-25.
XXII— 0	113.2	111.3	109.0	102.8	129.5	97.9	100.9	100.6	97.5	96.3	95.1	93.6
5	112.9	112.6			130.6	98.1	100.7	101.0	97.7	96.3	95.6	91.4
10	113.1	107.8			130.1	97.9	100.5	100.6	98.3	96.2	95.2	91.5
15	113.5	113.2			129.3	97.2	100.8	100.2	99.1	96.3	95.8	93.6
20	113.8	114.2	109.2	103.5	131.8	96.4	100.9	102.2	100.1	96.2	96.2	92.9
25	114.1	115.0			130.6	95.0	101.1	100.2	99.5	96.1	96.8	94.1
30	114.3	112.0			129.0	94.0	101.2	101.9	98.7	96.2	96.9	94.7
35	114.5	115.0			127.5	92.9	101.1	102.6	96.9	96.3	96.8	94.4
40	114.5	114.5	109.7	103.6	128.2	91.1	101.0	101.2	95.9	96.2	96.3	94.2
45	114.3	113.6			128.2	89.5	101.0	101.9	96.1	96.3	96.8	95.0
50	114.3	113.4			127.6	87.2	101.3	101.4	96.5	96.3	97.3	94.8
55	114.2	111.5			127.0	87.0	101.3	102.2	96.8	96.4	97.8	95.6
XXIII— 0	114.0	112.7	110.2	103.7	128.2	88.4	101.3	100.7	96.5	96.3	97.3	95.4
5	114.5	111.8			130.0	91.2	101.2	98.6	96.9	96.3	97.6	95.7
10	114.8	110.9			129.2	93.0	101.4	100.1	96.5	96.2	97.6	95.2
15	114.8	110.5			130.9	95.6	101.3	99.6	95.4	96.1	97.6	95.7
20	115.0	110.4	110.3	104.3	132.0	95.2	101.4	99.0	94.9	96.2	97.6	96.2
25	114.7	111.0			133.8	95.7	101.7	98.8	94.9	96.4	97.5	97.0
30	115.0	110.8			132.2	96.8	101.6	101.7	95.7	96.2	98.0	97.6
35	115.1	111.1			131.1	96.2	101.7	101.7	96.1	96.5	98.7	98.1
40	114.7	111.2	110.6	105.5	130.5	95.8	101.6	101.2	95.6	96.2	98.2	98.5
45	114.7	112.1		105.4	132.0	95.0	101.8	100.1	95.4	96.4	97.9	98.4
50	114.6	112.7			133.8	95.1	101.9	102.0	95.1	96.6	98.0	98.4
55	114.8	112.8			132.9	93.6	102.2	100.7	95.4	96.1	97.9	98.2
XXIV— 0	114.6	110.8	110.5	105.5	133.3	94.5	102.3	101.0	95.7	96.2	97.8	98.5
5	114.6	111.8	110.5		134.4	94.5	101.3	101.1	95.6	96.2	97.8	98.3
10	115.0	112.3			134.2	95.1	101.9	101.1	96.7	96.4	98.0	98.5
15	115.2	110.2			134.8	94.8	102.4	100.9	98.2	96.4	97.9	98.5
20	115.2	111.4	110.5	105.0	134.6	92.9	102.4	102.5	98.1	96.4	97.5	98.0
25	115.6	111.0			135.2	91.7	102.4	99.5	97.7	96.5	97.8	98.3
30	115.6	112.5			135.2	90.9	102.6	100.9	96.9	96.5	97.9	98.6
35	115.3	113.1			135.8	89.8	102.5	102.0	96.6	96.6	97.9	98.6
40	115.2	113.0	110.3	105.0	136.0	90.5	102.8	103.1	96.6	96.6	97.7	98.9
45	115.2	113.4			135.5	90.9	102.9	102.0	97.6	96.7	97.9	99.0
50	115.5	114.6			135.5	91.2	102.8	102.4	98.4	97.0	98.4	99.2
55	116.1	113.9			135.9	91.7	103.1	102.2	99.0	97.0	98.2	99.3
I— 0	115.7	113.5	109.4	104.8	136.5	90.3	103.0	102.5	99.4	97.2	98.6	100.0
5	115.2	112.0			135.9	90.7	103.5	101.5	100.3	97.3	98.4	100.0
10	116.0	112.5			135.3	92.6	103.6	101.2	100.5	97.2	98.8	99.9
15	116.4	111.9			135.0	92.4	103.6	101.1	100.0	97.3	98.9	99.8
20	117.4	111.4	108.1	103.1	135.7	93.4	102.5	101.7	99.4	97.6	98.9	99.7
25	116.5	110.4			134.5	95.4	103.5	102.0	99.7	97.8	99.2	99.4
30	117.1	109.2			133.6	97.2	103.5	102.0	101.5	97.8	99.6	99.8
35	117.3	108.8			132.6	97.8	103.6	102.5	100.0	97.9	99.8	100.1
40	117.0	108.7	107.2	102.0	131.2	99.2	103.6	102.2	99.5	97.8	99.9	99.8
45	116.7	108.9			130.8	100.7	103.3	101.5	98.0	97.8	100.0	100.2
50	116.7	108.1			130.8	101.6	103.4	101.5	98.1	97.9	99.5	100.1
55	116.6	107.7			130.5	101.7	103.3	101.3	99.6	98.0	99.3	99.7

TABLE I.—CONTINUED.

Gottingen Time, reconed from Mean Noon.	1840. March. 27-28.	May. 29-30.	June. 24-25.	July. 24-25.	Aug. 28-29.	Sept. 23-24.	Oct. 21-22.	Nov. 27-28.	Dec. 23-24.	1841. Jan. 20-21.	Feb. 25-27.	March. 24-25.
II— 0	117.0	107.0	107.4	101.3	129.9	101.6	104.3	101.7	99.2	97.5	99.5	100.6
5	116.7	105.6			128.2	99.6	102.9	102.6	98.8	98.1	98.9	99.9
10	116.6				129.0	99.7	103.8	103.7	99.9	98.3	98.7	99.9
15	116.2	107.5			128.6	98.0	103.3	103.9	99.8	97.9	98.5	99.3
20	115.9	108.5	107.0	101.6	128.0	99.8	103.3	103.5	100.9	97.9	98.2	98.7
25	115.6	107.1			127.1	99.7	103.3	103.3	100.6	98.2	97.9	99.5
30	115.6	105.4			126.1	101.5	104.1	102.3	99.3	98.7	98.5	99.2
35	114.9	103.6			125.3	101.1	104.0	101.8	98.0	98.6	98.7	99.7
40	114.8	104.2	106.5	102.7	125.7	100.4	103.4	102.1	98.0	98.4	98.4	99.6
45	114.5	103.9			124.2	99.4	102.7	101.3	97.6	98.3	98.4	99.4
50	114.5	102.9			125.0	99.3	102.6	101.5	98.6	98.4	98.4	98.9
55	114.7	103.4			123.5	97.2	102.2	101.2	100.3	98.1	98.6	98.7
III— 0	114.1	105.1	105.8	103.3	123.3	97.7	101.3	99.9	97.9	98.3	98.4	98.7
5	114.0	102.1			122.3	96.1	101.0	100.3	95.7	98.0	98.6	98.4
10	113.9	101.9			121.9	94.1	100.3	100.9	96.0	97.9	98.6	98.7
15	113.7	101.9			121.9	94.7	100.1	101.3	96.2	98.3	98.3	98.6
20	113.3	102.0	105.9	101.9	121.4	95.5	99.0	100.1	97.1	98.0	98.2	98.7
25	113.6	101.9			121.2	94.8	98.5	99.6	96.7	97.3	98.2	98.5
30	113.4	101.9			121.5	93.8	97.4	100.1	97.3	97.7	97.7	98.7
35	113.3	102.4			121.2	93.2	96.4	99.9	97.6	97.6	97.8	97.7
40	112.3		105.8	101.2	121.2	93.4	97.2	100.1	97.4	97.5	97.2	98.4
45	112.2				120.6	94.6	98.1	100.3	96.7	97.5	97.1	98.4
50	111.9				119.7	95.1	97.6	98.7	96.7	97.5	96.9	98.4
55	111.4				119.7	95.0	97.2	98.8	95.9	97.0	97.3	98.1
IV— 0	110.6	103.4	105.7	100.9	119.7	95.6	97.2	98.6	96.1	96.7	96.7	98.3
5	109.4	103.5			119.3	94.6	96.8	98.6	96.0	96.9	96.9	98.2
10	108.9				119.4	95.4	95.7	98.3	97.1	97.6	96.5	98.0
15	108.4	100.2			119.4	94.7	94.9	98.1	96.3	96.5	96.2	98.1
20	108.1		105.5	101.5	119.0	94.2	95.0	98.4	95.7	96.6	95.8	97.8
25	107.9				119.8	93.7	95.1	97.6	96.3	96.8	95.5	97.6
30	107.5	101.1			118.9	92.4	94.9	97.5	96.7	96.5	95.0	97.4
35	107.1				118.7	92.6	95.2	97.2	96.5	96.2	94.9	97.3
40	106.5		105.6	101.6	118.9	93.3	95.2	97.2	96.8	96.2	94.7	97.0
45	106.3	101.7			118.0	95.9	95.0	96.7	96.6	96.3	94.6	96.8
50	106.1				117.2	96.5	93.8	97.0	96.5	96.2	94.3	96.5
55	105.9				116.9	96.7	93.7	97.3	96.5	96.1	93.8	96.3
V— 0	105.8	102.0	105.4	101.4	116.7	96.3	93.6	97.6	96.0	96.1	93.6	96.0
5	105.8			101.5	116.9	96.9	93.5		95.9	96.1	93.7	95.8
10	105.9				117.0	97.8	93.4		96.4	95.8	93.7	95.6
15	105.9	101.0			115.8	98.4	93.4		96.2	95.9	93.1	95.4
20	106.0	102.0	105.0	101.0	115.4	98.3	93.6		96.0	96.4	92.6	95.1
25	105.8					97.8	93.6		95.7	95.9	93.1	95.1
30	105.8	102.0			115.0	98.0	93.9	97.9	95.7	95.7	93.1	95.0
35	105.9				115.1	98.1	94.1	97.5	95.3	95.5	92.8	94.6
40	105.8		104.1	101.4	114.8	98.2	94.0	97.5	95.4	95.2	93.2	94.5
45	105.7	102.2			114.8	98.6	94.6	97.3	94.6	94.9	92.8	94.2
50	105.8	101.7			114.8	98.8	95.0	97.4	95.0	94.4	92.5	93.6
55	105.8	102.0			114.5	98.8	95.1	97.6	94.6	94.2	92.5	93.6

TABLE I.—CONTINUED.

Gottingen Time, reckoned from Mean Noon.	1840. March. 27-28.	May. 29-30.	June. 24-25.	July. 24-25.	Aug. 28-29.	Sep. 23-24.	Oct. 21-22.	Nov. 27-28.	Dec. 23 24.	1841. Jan. 20-21.	Feb. 26-27.	March 24-25.
VI— 0	105.9	101.8	104.0	100.7	114.4	98.1	94.8	97.3	94.1	93.8	92.3	93.6
5	105.9				114.2	98.1	94.0	97.0	94.3	93.4	91.8	93.5
10	105.7	101.6		95.0	114.4	98.5	94.3	97.2	94.0	93.2	91.8	93.5
15	106.1			99.4	115.0	98.4	94.6	97.6	94.1	92.5	92.2	93.1
20	106.0	101.9	103.5	99.0	114.9	98.5	94.9	97.8	93.9	92.1	91.9	92.8
25	105.8			98.4	115.2	98.5	94.8	97.5	93.3	91.9	91.7	92.5
30	106.4	102.0			114.8	98.8	94.9	96.1	93.2	91.5	91.5	92.5
35	106.2	102.5		98.9	115.1	98.8	96.3	95.4	93.3	91.6	91.6	92.2
40	106.2	102.7	103.0	99.6	115.3	98.6	96.4	94.7	93.3	91.5	91.3	92.4
45	105.0	103.0			115.3	98.1	96.5	95.0	93.6	91.6	91.4	92.2
50	104.9	103.0			115.4	97.5	96.3	94.6	93.2	91.6	91.2	92.2
55	105.0	103.1			115.6	97.8	95.7	95.0	93.0	91.8	91.2	92.3
VII— 0	104.2	103.2	102.1	99.5	115.5	97.9	95.9	95.1	93.1	91.1	91.3	92.3
5	104.1	103.4			115.8	97.7	96.1	95.2	93.7	89.9	91.3	92.2
10	104.1	103.6			115.9	98.1	96.2	95.4	93.6	91.1	91.0	92.1
15	104.2	103.7			115.9	98.1	96.6	95.2	94.3	91.2	91.1	92.1
20	104.3	104.0	191.9	101.0	116.3	98.2	96.6	95.0	94.1	91.7	91.3	92.1
25	104.3	104.1			116.6	98.5	96.3	94.8	93.7	90.8	91.2	92.0
30	104.2	104.0			116.4	98.4	96.1	94.9	93.4	90.3	90.6	92.0
35	104.5	104.0			116.5	98.1	96.2	94.8	93.0	91.4	90.0	91.7
40	104.1	104.3	101.3	99.5	116.6	98.4	95.8	94.8	92.8	91.3	90.0	91.4
45	104.4	104.2			116.3	98.5	95.8	94.8	92.7	91.2	89.6	91.3
50	104.4	104.5			116.2	99.0	95.6	95.1	92.4	91.3	89.6	91.1
55	104.3	105.0			116.3	98.9	95.2	95.4	92.0	90.8	89.7	90.9
VIII— 0	104.2	105.0	101.2	99.3	116.7	99.3	95.2	95.4	91.8	90.4	89.9	91.0
5	104.1	105.2			116.7	100.2	95.2	95.6	92.2	90.2	90.2	91.0
10	104.1	105.9			116.8	100.2	95.1	95.6	92.0	90.3	90.9	90.8
15	104.2	105.6			117.0	100.7	94.9	95.6	92.1	90.1	91.1	90.8
20	104.3	105.5	100.9	98.4	116.9	101.5	94.9	95.7	92.3	90.9	91.2	91.1
25	104.3	105.6			117.2	103.4	94.9	96.0	91.8	90.7	91.2	90.9
30	104.2	105.5			117.1	104.1	94.9	95.7	91.9	90.9	91.4	90.7
35	104.5	105.5			118.6	104.8	94.9	95.8	91.4	90.0	91.5	90.1
40	104.1	106.0	100.9	97.8	118.0	105.7	95.1	95.7	91.6	89.5	91.5	90.0
45	104.4	106.2			116.9	106.2	95.1	95.6	91.7	87.8	91.6	89.9
50	104.4	107.0			117.5	107.1	94.9	95.3	91.7	87.0	91.6	90.0
55	104.3	107.5			117.5	107.6	94.8	94.8	92.5	87.8	91.9	89.9
IX— 0	104.6	107.1	100.4	97.2	117.6	107.4	95.5	94.7	92.6	87.9	91.9	89.9
5	104.9	107.5			117.3	106.0	95.4	95.0	92.5	88.1	91.8	89.9
10	104.7	106.7			117.5	104.8	95.6	95.3	92.4	88.4	92.1	89.8
15	105.3	106.2			117.6	104.1	95.7	95.7	92.5	89.7	92.3	89.8
20	105.4		101.0	98.7	117.8	103.8	96.1	96.0	93.5	91.0	92.2	89.9
25	105.4	107.0			117.4	102.5	96.2	96.6	93.8	91.5	92.4	90.2
30	105.4	108.9			117.5	102.8	96.4	96.8	94.0	90.8	92.3	90.7
35	105.7	107.4			117.3	101.0	96.9	97.2	93.9	92.1	92.4	91.1
40	105.9	108.9	101.0	99.2	117.4	101.0	97.1	97.5	94.0	91.9	92.3	91.3
45	105.8	108.2			117.5	102.1	97.4	97.6	93.7	92.2	92.5	91.5
50	106.1	113.0			117.4	102.5	97.7	98.7	93.9	92.3	92.6	91.6
55	106.6	113.2			117.7	102.6	98.0	98.6	94.0	93.2	92.8	92.1

That part of Table II. which is referred to on page 24 may be found on page 38, where it is introduced for another purpose. The column containing the greatest daily excursions, which was promised on page 51, has been omitted altogether, as it did not seem to be necessary for a complete view of the subject under discussion.

TABLE III.

	JUNE.			AUGUST.			SEPTEMBER.			OCTOBER.		
	Com- puted.	Ob- served.	Differ- ence.	Com- puted.	Ob- served.	Differ- ence.	Com- puted.	Ob- served.	Differ- ence.	Com- puted.	Ob- served.	Differ- ence.
X.												
0	102.57	102.67	— .10	118.00	118.01	— .01	103.95	105.52	— 1.57	97.37	97.04	+ .33
20	102.93	103.01	— .08	118.27	118.45	— .18	104.05	107.54	— 3.49	97.86	98.41	— .55
40	103.24	103.15	+ .09	118.51	118.95	— .44	104.16	107.16	— 3.00	98.34	100.36	— 2.02
XI.												
0	103.42	103.35	+ .07	118.72	119.22	— .50	104.29	105.45	— 1.16	98.80	100.18	— 1.38
20	103.65	103.54	+ .11	118.92	119.43	— .51	104.46	106.36	— 1.90	99.23	99.29	— .06
40	103.73	103.72	+ .01	119.11	119.47	— .36	104.66	103.81	+ .85	99.63	99.78	— .15
XII.												
0	103.72	103.61	+ .11	119.30	119.09	+ .21	104.89	101.39	+ 3.50	99.98	98.94	+ 1.04
20	103.63	103.85	— .22	119.49	119.46	+ .03	105.14	100.59	+ 4.55	100.36	98.72	+ 1.64
40	103.49	103.71	— .22	119.69	119.23	+ .46	105.38	101.28	+ 4.10	100.55	101.87	— 1.32
XIII.												
0	103.26	103.34	— .08	119.90	119.18	+ .72	105.59	104.16	+ 1.43	100.76	100.62	+ .14
20	103.16	103.45	— .29	120.13	119.35	+ .78	105.74	106.31	— .57	100.92	101.23	— .31
40	103.03	103.12	— .09	120.37	120.15	+ .22	105.83	107.54	— 1.71	99.04	101.36	— 2.32
XIV.												
0	102.97	102.90	+ .07	120.62	120.27	+ .35	105.83	111.59	— 5.76	101.11	100.11	+ 1.00
20	102.99	102.53	+ .41	120.88	118.98	+ .90	105.75	108.57	— 2.82	101.13	100.15	+ 0.98
40	103.12	102.31	+ .81	121.14	121.48	— .34	105.57	106.07	— .50	101.12	101.57	— .45
XV.												
0	103.36	103.27	+ .09	121.40	122.24	— .84	105.33	104.98	+ .35	101.06	101.27	— .21
20	103.72	104.28	— .56	121.64	121.59	+ .05	105.02	104.23	+ .79	100.98	100.85	+ .13
40	104.11	104.72	— .61	121.85	122.24	— .39	104.69	102.99	+ 1.70	100.86	101.76	— .90
XVI.												
0	104.58	104.78	— .20	122.08	123.18	— 1.15	104.35	103.68	+ .67	100.72	103.37	— 2.65
20	105.07	105.46	— .39	122.18	123.29	— 1.11	104.04	104.13	— .09	100.54	98.23	+ 2.31
40	105.55	105.94	— .39	122.29	122.94	— .65	103.77	103.78	— .01	100.34	98.82	+ 1.52
XVII.												
0	105.97	105.76	+ .21	122.35	122.69	— .34	103.56	99.78	+ 3.78	100.12	100.80	— .68
20	106.31	106.21	+ .10	122.38	122.67	— .29	103.42	104.46	— 1.04	99.87	100.89	— 1.02
40	106.55	106.20	+ .35	122.39	121.90	+ .49	103.35	102.52	+ .83	99.62	100.24	— .62
XVIII.												
0	106.69	106.28	+ .41	122.36	121.25	+ 1.11	103.35	103.96	— .61	99.36	98.05	+ 1.31
20	106.72	106.49	+ .23	122.33	120.95	+ 1.38	103.40	105.33	— 1.93	99.10	98.53	+ .57
40	106.66	106.27	+ .39	122.30	121.74	+ .56	103.48	102.52	+ .96	98.84	99.23	— .39
XIX.												
0	106.55	106.48	+ .07	122.29	121.90	+ .39	103.57	105.22	— 1.65	98.60	98.51	+ .09
20	106.38	106.45	— .07	122.30	122.62	— .32	103.65	104.16	— .51	98.38	98.93	— .55
40	106.24	106.34	— .10	122.36	121.50	+ .86	103.70	104.60	— .90	98.20	99.45	— 1.25
XX.												
0	106.05	106.95	— .90	122.46	122.35	+ .11	103.71	105.72	— 2.01	98.07	98.81	— .74
20	106.15	106.78	— .63	122.63	123.61	— .98	103.67	104.39	— .72	97.99	97.78	+ .21
40	106.23	106.68	— .45	122.86	123.72	— .86	103.58	102.40	+ 1.18	97.98	97.51	+ .47
XXI.												
0	106.46	107.03	— .57	123.14	123.86	— .72	103.45	101.92	+ 1.53	98.03	95.76	+ 2.27
20	106.79	106.84	— .05	123.48	123.78	— .30	103.28	104.65	— 1.37	98.14	96.98	+ 1.16
40	107.24	107.20	+ .04	123.87	123.90	— .03	103.12	103.81	— .69	98.33	98.74	— .41
XXII.												
0	107.77	107.16	+ .61	124.28	124.47	— .19	102.95	101.47	+ 1.48	98.58	99.54	— .96
20	108.46	107.68	+ .78	124.70	124.77	— .07	102.79	99.50	+ 3.29	98.89	99.74	— .85
40	109.06	108.32	+ .74	125.12	124.60	+ .52	102.66	100.80	+ 1.86	99.24	100.20	— .96

TABLE III.—CONTINUED.

	JUNE.			AUGUST.			SEPTEMBER.			OCTOBER.		
	Com- puted.	Ob- served.	Differ- ence.	Com- puted.	Ob- served.	Differ- ence.	Com- puted.	Ob- served.	Differ- ence.	Com- puted.	Ob- served.	Differ- ence.
XXIII.												
0	109.53	109.02	+ .51	125.50	124.61	+ .89	102.56	104.86	-2.30	99.62	100.53	-.91
20	110.20	109.55	+ .65	125.82	126.11	-.29	102.48	107.79	-5.31	100.00	100.50	-.50
40	110.37	109.86	+ .51	126.07	126.56	-.49	101.24	103.32	-.91	100.39	99.42	+ .97
XXIV.												
0	110.58	110.31	+ .27	126.22	126.18	+ .04	102.34	103.35	-1.01	100.74	99.71	+ 1.03
20	110.63	110.53	+ .10	126.25	126.08	+ .17	102.24	102.46	-.22	101.04	99.80	+ 1.24
40	110.50	110.19	+ .31	126.15	125.70	+ .45	102.10	100.72	+ 1.38	101.28	100.94	+ .34
I.												
0	110.21	109.51	+ .70	125.92	126.22	-.30	101.90	99.74	+ 2.16	101.44	101.47	-.03
20	109.86	109.14	+ .72	125.56	125.18	+ .38	100.62	99.85	+ .77	101.50	101.41	+ .09
40	109.28	108.67	+ .61	125.06	124.95	+ .11	100.26	101.33	-1.07	101.46	101.69	-.23
II.												
0	108.51	108.23	+ .28	124.44	124.42	+ .02	100.80	101.21	-.41	101.31	102.25	-.94
20	107.78	107.74	+ .04	123.71	123.79	-.08	100.29	100.16	+ .13	101.06	101.92	-.86
40	107.02	107.15	-.13	122.90	123.00	-.10	99.71	99.95	-.24	100.70	101.88	-1.18
III.												
0	106.27	106.75	-.48	122.04	122.25	-.21	99.12	99.60	-.48	100.26	100.76	-.50
20	105.54	106.15	-.61	121.13	121.05	+ .08	98.54	98.47	+ .07	99.74	99.94	-.20
40	104.86	105.28	-.42	120.22	120.43	-.21	98.03	98.51	-.48	99.16	98.83	+ .33
IV.												
0	104.34	104.28	+ .06	119.34	119.90	-.56	97.60	98.17	-.57	98.56	98.13	+ .43
20	103.69	103.69	.00	118.51	118.68	-.17	97.31	97.81	-.50	97.93	96.85	+ 1.08
40	103.19	103.07	+ .12	117.75	117.75	.00	97.19	96.94	+ .25	97.31	97.08	+ .23
V.												
0	102.75	102.66	+ .09	117.09	117.20	-.11	97.20	96.52	+ .68	96.72	96.04	+ .68
20	102.35	102.25	+ .10	116.53	116.45	+ .08	97.41	97.81	-.40	96.17	95.93	+ .24
40	102.00	101.87	+ .13	116.09	116.03	+ .06	97.67	97.95	-.28	95.70	95.59	+ .11
VI.												
0	101.60	101.64	-.04	115.78	115.85	-.07	98.28	98.06	+ .22	95.30	95.38	-.08
20	101.43	101.35	-.08	115.59	115.87	-.28	98.90	99.18	-.28	94.99	95.54	-.55
40	101.21	101.40	-.19	115.52	115.34	+ .18	99.60	100.17	-.57	94.79	95.58	-.79
VII.												
0	101.04	100.98	+ .06	115.55	115.18	+ .37	100.31	100.63	-.32	94.68	95.22	-.54
20	100.93	100.87	+ .06	115.68	115.37	+ .31	101.03	100.00	+ 1.03	94.68	95.57	-.89
40	100.89	100.89	.00	115.88	115.69	+ .19	101.69	100.92	+ .77	94.74	95.53	-.79
VIII.												
0	100.93	101.20	-.27	116.14	116.18	-.04	102.28	102.89	-.61	94.96	95.15	-.19
20	101.05	101.24	-.19	116.44	116.26	+ .18	102.78	102.26	+ .52	95.23	94.38	+ .85
40	101.26	101.34	-.08	116.76	116.35	+ .41	103.18	102.08	+ 1.10	95.57	94.84	+ .73
IX.												
0	101.51	101.28	+ .23	117.09	116.84	+ .25	103.48	102.43	+ 1.05	95.97	94.96	+ 1.01
20	101.84	101.66	+ .18	117.41	117.47	-.06	103.69	101.63	+ 2.06	96.41	95.54	+ .87
40	102.19	102.30	-.11	117.72	118.18	-.46	103.84	103.38	+ 1.46	96.88	96.14	+ .74

This Table gives four monthly mean observed places of the magnetometer for every 20 min during the day. A parallel column gives the corresponding place as calculated by the empirical curve. The column of differences shews to what extent the empirical and observed places agree, and the sum of these differences make it probable that they are accidental. The numbers in all these Tables are in minutes and decimal parts of a minute, according to the scale of the Gauss Magnetometer.

PROCEEDINGS OF THE ROYAL IRISH ACADEMY.

From the Philosophical Magazine.

Nov. 9, 1840.—The Rev. T. R. ROBINSON, D.D., M.R.I.A., gave the Academy an account of a large reflecting telescope, lately constructed by Lord Oxmantown, and of the processes employed in forming its specula.*

After explaining the relative importance of magnifying and illuminating power, Dr. R. proceeded to give a brief sketch of the history of the reflecting telescope, which seemed to have been forgotten for many years after its invention, till it was revived by Hadley. The labours of Short soon gave it celebrity; yet even this artist limited himself in almost every instance to sizes which were not more powerful than the achromatics of his day, and his large instruments appear to have been failures.† It was not till a full century after the publication of Newton's paper that Sir William Herschel gave this telescope the gigantic developement which has crowned him with imperishable fame; and by the construction of telescopes of nineteen and forty-eight inches aperture, placed regions almost beyond the scope of measurement within the reach of human intellect. But as Short, in a spirit unworthy of his talents, took care that his knowledge should die with himself, and Herschel published nothing of the means to which his success was owing, the construction of a large reflector is still as much as ever a perilous adventure, in which each individual must grope his way. Accordingly, the London opticians themselves do not like to attempt a mirror even of nine inches diameter, and demand a price for it, which shews the uncertainty and difficulty of its execution. In Ireland we are more fortunate, for a member of our Academy, Mr. Grub, finds no difficulty in making them of admirable quality up to this size, or even fifteen inches; but with all his distinguished mechanical talent, he is believed to be doubtful of the possibility of more than doubling this last magnitude in perfect speculum metal.

Under these circumstances, too much praise cannot be given to Lord Oxmantown, who, in the midst of other pursuits, has found leisure for such researches; and by a rare combination of optical science, chemical knowledge, and practical mechanics, has given us the power of overcoming the difficulties which arrested our predecessors, and of carrying to an extent which even Herschel himself did not venture to contemplate, the illuminating power of this telescope, along with a sharpness of definition scarcely inferior to that of the achromatic.

* An abstract of Lord Oxmantown's paper on his telescope, read before the Royal Society, and published in the "*Philosophical Transactions*," will be found in "*Phil. Mag.*," Third Series, vol. xvii. p. 380.

† A Newtonian of six feet focus, and 9.4 inches aperture, is said by Maskelyne to have shewn the first satellite of Jupiter 13' longer than a *triple* achromatic of 3.6 inches aperture. The telescope of twelve feet focus, and eighteen inches aperture, now at Oxford, shewed multiple rings of Saturn.

The chief difficulties which are to be overcome in the construction of reflectors, arise from the excessive brittleness of the composition of which specula are made, and from the necessity of giving them figures which shall be free from aberration. The great mirror in the Newtonian form is (if the eyepiece and plane mirror be correct) the conical paraboloid.

It is necessary that speculum metal should possess, in the highest attainable degree, the qualities of whiteness, brilliancy, and resistance to tarnish. Lord Oxmantown has found that these conditions are best satisfied in the *definite* combinations of four equivalents of copper to one of tin; or by weight, 32 and 14·7 nearly. Metals differing from this by a slight excess of either component, are, when first polished, scarcely less brilliant, but are dimmed so rapidly that the lapse of a few days produces a sensible difference. On the other hand, some large specula of the atomic compound have been lying uncovered for years, without material injury to their polish.

But this compound is brittle almost beyond belief; a slight blow, or even the application of partial warmth, will shiver a large mass of it; though harder than steel, its surface is broken up with the utmost facility, and it has a most energetic tendency to crystallize. The common process of the founder fails with it, except for masses of very limited magnitude, as the cast cracks in the mould; and the subsequent difficulties of the annealing are such, that it has been a very general practice to use an alloy lower (containing more copper) than the atomic standard. Even Sir William Herschel was obliged to yield to this necessity. It appears from a letter of Smeaton, (Rees's Cyclopædia, Art. Telescope,) that for his 20-foot mirror of 19 inches aperture, the composition was 32 copper to 12·4 tin, and that for the 40-foot it was even lower; yet two out of three attempts to cast this huge speculum failed.

Lord Oxmantown at first endeavoured to evade the difficulty, by constructing a speculum in pieces, soldering plates of fine metal to a back of a peculiar brass, ascertained to have the same expansion; and has completed one of thirty-six inches aperture and twenty-seven feet focal length, which performs very well on stars below the fifth magnitude, but above that exhibits a cross formed by the diffraction of the joints; and in unsteady states of the air exhibits the sixteen divisions of the great mirror on the star's disc. By diminishing the number and size of the joints, it is found that these inconveniences can be diminished, so as to be scarcely perceptible; and in all probability this is the process by which the remotest limits of telescopic vision will ultimately be attained. It is, however, not necessary for instruments of even greater dimensions than this, since Lord Oxmantown has succeeded, by a contrivance as simple as ingenious, in casting at the first attempt a *solid* mirror of the same size; and there is no reason to suppose that it will be less effective on a much larger scale.

But however difficult it may be to obtain the rough speculum of large dimensions, it is still more so to give it a proper figure, combined with that brilliant polish which is technically called black, because it reflects no light out of the plane of incidence. In such mirrors as can be wrought by hand, they are worked by short cross strokes on the polisher, and at the same time have a slow rotation relative to it. This might be expected to produce merely a spherical figure; but by varying the length of the stroke, by circular movement, elliptic figure of the polisher, or removing portions of its pitch covering, a parabolic figure is obtained. For sizes above nine inches diameter, the work must be performed by machinery; but in all which Dr. Robinson has seen (the most remarkable of which are those of Sir William Herschel* and Mr. Grubb,) the cross stroke is given by a lever moved by hand: and it is supposed that perfect results cannot be obtained but by the *feeling* of the polisher's action. Sir John Herschel is believed to have made important additions to his father's apparatus; and it is to be hoped he will soon redeem his promise (Mem. R. Ast. Soc., vol. vi.) of publishing his improvements.

Lord Oxmantown has in many respects deviated from the usual process. His polisher, of the mirror's diameter, intersected by transverse and circular grooves into portions not exceeding half an inch of surface, is coated, first, with a thin layer of the common optical pitch, and then with a much harder compound. It is worked *on* the mirror, and counterpoised, so that but little of its weight bears; but the want of pressure is compensated by a long and rapid stroke. The mirror revolves slowly in a cistern of water, maintained at an uniform temperature, to prevent the extrication of heat by friction. The polisher moves slowly in the same direction, while it is also impelled with two rectangular movements. The machine is driven by steam, and requires no superintendence, except to supply occasionally a little water to the polisher, and to watch when the polish is complete. By an induction from experiments on mirrors from six to thirty-six inches aperture, it was found that if the magnitudes of the transverse movements be $\frac{1}{3}$ rd and $\frac{2}{100}$ th of the aperture, and their times be to its period of rotation as 1 and 1.8 to 37, the figure will be parabolic: but to combine with this the highest degree of lustre, it is found necessary to apply, towards the close, a solution of soap in liquid ammonia, which seems to exert a specific action.

The certainty of the process is such, that the solid mirror of thirty-six inches aperture, after being scratched all over its surface with coarse putty, was, in Dr. Robinson's presence, perfectly polished in about six hours, and was placed in its tube for examination without any previous trial as to quality.

Lord Oxmantown has preferred the Newtonian to the Herschelian form, and, in Dr. Robinson's opinion, with good reason. In the latter

* Dr. Robinson had the good fortune to see this at Slough, in 1830, while at work on a twenty-foot mirror.

the inclination of the great mirror to the incident rays must deform the image,* and it is now known, that even with faint objects sharp definition is of high importance. It should, in fact, be a segment of a paraboloid, exterior to the axis; and though a theorem of Sir William Hamilton (*Trans. Roy. Irish Acad.*, vol. xv. p. 97) might seem to indicate mechanical means of approximating to the figure, yet Dr. Robinson fears there would be greater difficulty in applying them than in enlarging the aperture of the Newtonian, so as to make up for the loss of light. Another serious objection is, that in the Herschelien the observer's position at the mouth of the tube must cause currents of heated air, which will materially interfere with sharpness of definition.

As to the loss of light by the second reflexion, Dr. Robinson thinks it has been much overrated, and expresses a wish that a careful set of experiments were made on reflection by plain specula at various incidences, on prisms of total reflection, and the achromatic prism, produced as a substitute by Sir David Brewster.

As to the rest of the instrument, it may suffice to say, that it bears a general resemblance to that of Ramage, but that the tube, gallery, and vertical axis of the stand are counterpoised, so that one man can easily work it, notwithstanding its enormous bulk. The specula, when not in use, are preserved from moisture or acid vapours, by connecting their boxes with chambers containing quick lime, which is occasionally renewed. This arrangement (which also occurred to Dr. Robinson, and has been for several years applied by him to the Armagh reflector) appears to be very effective in preserving the polish.

In trying the performance of the telescope, Dr. Robinson had the advantage of the assistance of one of the most celebrated of British astronomers, Sir James South; but they were unfortunate in respect to weather, as the air was unsteady in almost every instance; the moonlight was also powerful on most of the nights when they were using it. After midnight, too, (when large reflectors act best,) the sky, in general, became overcast. The time was from October 29th to November 8th.

Both specula, the divided and the solid, seem exactly parabolic, there being no sensible difference in the focal adjustment of the eyepiece with the whole aperture of thirty-six inches, or one of twelve; in the former case there is more flutter, but apparently no difference in definition, and the eyepiece comes to its place of adjustment very sharply.

The solid speculum shewed α Lyræ round and well defined, with powers up to 1000 inclusive, and at moments even with 1600; but the air was not fit for so high a power on any telescope. Rigil, two

* Any one who has a Newtonian telescope can verify this, by inclining a little the great mirror, so however as not to pass the edge of the plane mirror by the pencil. In Lord Oxmantown's instrument, an inclination of $11'$ sensibly injures it; were it Herschelien, the inclination must be $3'' 11'$.

hours from the meridian, with 600, was round, the field quite dark, the companion separated by more than a diameter of the star from its light, and so brilliant that it would certainly be visible long before sunset. ζ Orionis, well defined, with all the powers from 200 to 1000, with the latter a wide black separation between the stars; 32 Orionis and 31 Canis minoris were also well separated.

It is scarcely possible to preserve the necessary sobriety of language, in speaking of the moon's appearance with this instrument, which discovers a multitude of new objects at every point of its surface. Among these may be named a mountainous tract near Ptolemy, every ridge of which is dotted with extremely minute craters, and two black parallel stripes in the bottom of Aristarchus.

The Georgian was the only planet visible; its disc did not shew any trace of a ring. As to its satellites, it is difficult to pronounce whether the luminous points seen near it are satellites or stars without micrometer measures. On October 29th, three such points were seen within a few seconds of the planet, which were not visible on November 5th; but then two others were to be traced, one of which could not have been overlooked in the first instance, had it been in the same position. If these were satellites, as is not improbable, there would be no *great* difficulty in taking good measurement both of their distance and position.

There could be little doubt of the high illuminating power of such a telescope, yet an example or two may be desirable. Between ϵ^1 and ϵ^2 Lyræ, there are two faint stars, which Sir J. Herschel (Phil. Trans. 1824) calls "*debilissima*," and which seem to have been, at that time, the only set visible in the twenty-foot reflector. These, at the altitude of 18° , were visible *without an eye-glass*, and also when the aperture was contracted to twelve inches. With an aperture of eighteen inches, power 600, they and two other stars (seen in Mr. Cooper's achromatic of 13.2 aperture, and the Armagh reflector of 15) are easily seen. With the whole aperture, a fifth is visible, which Dr. Robinson had not before noticed. Nov. 5th, strong moonlight.

In the nebula of Orion, the fifth star of the trapezium is easily seen with either speculum, even when the aperture is contracted to eighteen inches. The divided speculum will not shew the sixth with the whole aperture, on account of that sort of disintegration of large stars already noticed, but does, in favourable moments, when contracted to eighteen inches. With the solid mirror and whole aperture, it stands out conspicuously under all the powers up to 1000, and even with eighteen inches is not likely to be overlooked.

Comparatively little attention was paid to nebulae and clusters, from the moonlight, and the superior importance of ascertaining the telescope's defining power. Of the few examined were 13 Messier, in which the central mass of stars was more distinctly separated, and the stars themselves larger than had been anticipated; the great nebula of Orion and that of Andromeda shewed no appearance of

resolution ; but the small nebula near the latter is clearly resolvable. This is also the case with the ring-nebula of Lyra ; indeed Dr. Robinson thought it was resolved at its minor axis : the fainter nebulous matter which fills it is irregularly distributed, having several stripes or wisps in it ; and there are four stars near it, besides the one figured by Sir John Herschel in his catalogue of nebulae. It is also worthy of notice, that this nebula, instead of that regular outline which he has there given it, is fringed with appendages, branching out into the surrounding space, like those of 13 Messier, and in particular, having prolongations brighter than the others in the direction of the major axis, longer than the ring's breadth. A still greater difference is found in 1 Messier, described by Sir John Herschel as "a barely resolvable cluster," and drawn fig. 81, with a fair elliptic boundary. This telescope, however, shews the stars as in his figure 89, and some more plainly, while the general outline, besides being irregular and fringed with appendages, has a deep bifurcation to the south.

From these and some other discrepancies, Dr. Robinson thinks it of great importance that the globular nebulae and clusters should be all carefully reviewed, as it is chiefly from their supposed regularity that the hypothesis of the condensation of nebulous matter into suns and planets has arisen ; an hypothesis which he thinks has, in some instances, been carried to an unwarrantable extent.

On the whole, he is of opinion that this is the most powerful telescope that has ever been constructed. So little has been published respecting the performance of Sir W. Herschel's forty-foot telescope, that it is not easy to institute a comparison with *that*, the only one that can be fairly made to compete with it. But there are two facts on record which lead to the inference that it was deficient in defining power : one, the low power used, which Dr. Robinson thinks was not above 370 ; the other, the circumstance that neither the fifth nor sixth stars of the trapezium of the nebula of Orion were shewn by it. As to light, there is no reason to believe that the composition of the forty-foot mirror was as reflective as that of the twenty-foot ; and if Dr. Robinson be correct in the opinion, that the latter* did not shew the fifth star easily, or the sixth at all, and that it only exhibited the "debilissima" and one star near the ring-nebula, then *it* has decidedly less illuminating power than eighteen, perhaps not more than fourteen inches aperture of Lord Oxmantown's mirror, notwithstanding the loss of light in that by the reflection at the second speculum.

However, any question about this optical pre-eminence is likely soon to be decided, for Lord Oxmantown is about to construct a telescope of unequalled dimensions. He intends it to be six feet aperture and fifty feet focus, mounted in the meridian, but with a range of about half an hour on each side of it. If he succeeds in giving it the same degree of perfection as that which he has attained

* In its original state, not as improved by the more perfect means latterly employed by Sir John Herschel.

in the present instance, which is exceedingly probable, it will be indeed a proud achievement ; his character is an assurance that it will be devoted, in the most unreserved manner, to the service of astronomy ; while the energy that could accomplish such a triumph, and the liberality that has placed his discoveries in this difficult art within reach of all, may justly be reckoned among the highest distinctions of Ireland.

November 30.—DR. KANE read a paper “On the Production of Audible Sounds.”

The following note, “On the Course of the Diurnal Fluctuations of the Barometer,” by JAMES P. ESPY, A.M., of Philadelphia, was communicated by DR. APJOHN.

“It is a law of inertia, that if a body is forced upwards, it will react and press on its support more than its natural gravity ; and if it is permitted to descend, it will press on its support less than its natural gravity, and the increase and diminution of pressure will be proportional to its velocity.

“Moreover, if a body is permitted to descend with a certain velocity, and then retarded, it will, when retarded, press more on its support than its natural gravity, and that in proportion to the rapidity of its retardation.

“This principle will explain the four fluctuations of the barometer which occur every day.

“Just before sunrise, when the atmosphere is neither becoming hotter nor colder, the barometer will indicate the natural weight of the air, which we may call a mean ; as the sun rises the air will begin to expand by heat, and the whole atmosphere will be lifted up by this expansion, and by its reaction will cause the barometer to rise ; and this will be the greatest at the time when the air is receiving the most rapid accessions of heat, which must take place before the hottest time of the day, when the air is becoming neither hotter nor colder. On this principle, then, the maximum day fluctuation will take place between daylight in the morning and the hottest time of the day, and this corresponds with the fact ; for this maximum, which amounts to more than the tenth of an inch, takes place about nine or ten o'clock A.M.

“At the hottest part of the day, when the air is neither expanding nor contracting, it is manifest that the barometer will stand again at a mean. Soon after this, however, the air will begin to contract from diminishing temperature, and at the moment of the most rapid acceleration of contraction, the barometer will stand at its day minimum, which will probably be late in the afternoon ; and it is found in fact to be from four to five o'clock. From this time the rapidity of the downward motion of the air from contraction begins to diminish, and the barometer of course begins to rise ; and at the moment when it is most rapidly retarded in its contraction, the barometer will be at its maximum night fluctuation, and will again be above the mean, but not so much as the day maximum.

"This maximum is found to occur about ten or eleven o'clock P.M. The air will now go on contracting more and more slowly, until about daylight, when it will be at rest, and the barometer will again be at a mean.

"This theory was given by me to the Journal of the Franklin Institute, and published ten or twelve years ago.

"I ventured in that paper to predict, notwithstanding some alleged observations at St. Bernard's Hospital to the contrary, that it would be found by more careful observations that the morning maximum fluctuation would be greater in lofty situations, on the sides of mountains, provided they were not very lofty, than on the plain below.

"For it is manifest, that there will be not only a reaction at these lofty situations, (a little less, it is true, than below,) but some of the air will be lifted up, by the expansion of the air below, above the upper place of observation; which would in all probability more than compensate the diminished reaction at moderate elevations.

"This prediction has been entirely verified by Lieutenant-Colonel Sykes's observations in India, and this verification may be considered as a strong proof of the correctness of the theory. It is quite probable, that maximum day fluctuation occurs later at considerable elevations than on the plain below.

"The theory would lead us also to suppose, that at very *great elevations*, where the reaction is very minute, only two fluctuations would be found in the day: the maximum at about two o'clock P.M., when most air would be above the barometer; and the minimum at daylight in the morning, when least air would be above it; but I know of no observations to confirm or refute these deductions."

Mr. BALL brought under the notice of the Academy the fact, that the ordinary sturgeon of the Dublin markets is an undescribed species. He stated that Mr. Thompson, of Belfast, and Professor Agassiz, concurred with him in this opinion, and he proposed to call it *Accipenser Thompsoni*, purposing, if permitted, to give figures and full descriptions in a future number of the "Proceedings."

The Archbishop of Dublin made some observations on a remarkable meteor, lately seen in different parts of Britain.

December 14.—Dr. Apjohn read the following notice, by George J. KNOX, Esq., of "some Improvements in the Voltaic Pile."

"The chief imperfection in the voltaic pile—its want of a constant uniform power of long duration, by which it is rendered almost useless as an instrument of research—having been overcome by the ability of Professor Daniell, the only thing that remained to render it efficient seemed to be, to increase its power; a desideratum accomplished by Mr. Grove, by substituting for copper and sulphate of copper, platinum and nitric acid.

"The following experiments were undertaken with the intention of estimating the relative values of the different constructions of Grove's battery, recommended by Mr. Knight, of Foster Lane, as

far as respects the arrangement of the zinc and platina plates, when, to my surprise, I found the *same* quantity of electricity to be evolved when the zinc is bent so as to expose an opposing surface to *each* surface of a platinum plate, as when a platinum plate, of the size of the former zinc, is similarly placed with respect to a plate of zinc of the same size as the former platinum, affording an economical method of arranging a Wollaston's battery, the zincs being bent round the coppers, in place of the coppers round the zincs.

"*Experiments with Grove's Battery.*—The acid solutions were those recommended by Mr. Grove, i. e. pure nitric acid, in contact with the platina; sulphuric acid + 4.5 water by measure, in contact with the zinc. The surfaces of zincs immersed were 3 by 2.5 inches; those of the platina, bent round the porous vessel holding the zincs, were 6 by 2.5 inches. The glasses containing the acid, &c., were 3.2 inches long, 1.5 broad, 3.5 deep. The length of the porous vessel of pipeclay was 2.5 inches, the breadth 0.3, the depth 3.5. The number of alternations was five.

	Cubic inches.
Time, 2 minutes	8.0
The battery being at rest for 10 minutes	8.0
.. .. 25	3.0
.. .. 1 hour	1.0
.. .. 19 hours	none

"The porous vessel was found filled with sulphate of zinc, which stopped the action of the battery.

"*Second Experiment.*—The zinc plates being of the same size as the former platina, and the platina of the same size as the former zincs; the zincs bent round the platina; all other things being as before.

	Cubic inches.
Time, 2 minutes	8.
After 10 minutes	8.
.. 25	8.

"*Third Experiment.*—Another battery, the diameter of the cells of which was $2\frac{1}{2}$ inches, gave a diminution of only *one-half* of the quantity of gas after the lapse of *forty-eight hours*, shewing the advantage of having a large supply of sulphuric acid.*

"*Experiments with Smee's Battery of Platinized Silver.*—The acid solution was of the same strength as before, and the sizes of the zincs and platinized silver the same as of the zincs and platina formerly employed. The zincs were bent round the platina.†

* "The porous vessels were of pipeclay. The same experiments repeated with unglazed porcelain, gave 10 cubic inches in two minutes; with very porous pipeclay, they gave as much as 15 cubic inches in two minutes, shewing the importance of attending to the nature of the porous vessel employed."

† "The most advantageous method of arranging a Smee's battery is packing the zincs and platinized silvers in the manner recommended by Dr.

	Cubic inches.
Time, 2 minutes	3'
After 5 minutes	2'6

"*Second Experiment.*—The zincs being raised out of the acid, cut in two, and re-immersed.

	Cubic inches.
Time, 2 minutes	1'6
After 5 minutes	1'6

"*Third Experiment.*—The zincs and platinized silver being removed, the acid remaining untouched; the platinized silver plates were bent round the zincs.

	Cubic inches.
Time, 2 minutes	2'6
After 5 minutes	2'4

"*Fourth Experiment.*—The platinized silver cut in two.

	Cubic inches.
Time, 2 minutes	1'4
After 5 minutes	1'4

"Supposing, from these experiments, the same quantity of electricity to be developed, whichever of the opposed surfaces of the two metals be the greater, I placed in separate glasses five zinc cylinders, one-inch diameter, immersed eight-tenths of an inch in the acid; platina foil, connected by binding screws with the zincs, was rolled into cylinders two-tenths of an inch in diameter, and then immersed in pipeclay tubes one inch deep.

	Cubic inches.
Time, 2 minutes	1'0
After 10 minutes	1'0

"*Second Experiment.*—Platina foil of the same size as the zincs, and zinc rods of the same diameter as the platina cylinders, being employed, the effects were precisely the same.

	Cubic inches.
Time, 2 minutes	1'0
After 10 minutes	1'0

"*Third Experiment.*—The zinc cylinders being made twice the diameter of the former, the quantity of gas generated in two minutes was the same as before, the increased number of lines of electrical force compensating the increased resistance offered by the acid solution.

"*Fourth Experiment.*—With cylinders twice the diameter of these, a very feeble current passed, the obstacle being too great to be overcome; by increasing the diameter of the porous vessels, and thereby of the nitric acid solution, which is a good conductor, the impediment is diminished, as shewn in experiment fifth. Thus Mr.

Faraday in his tenth series, (also by Mr. Young, Phil. Mag., s. 3, vol. x., p. 241) placing the package on supports so as to allow the sulphate of zinc to fall to the bottom of the vessel, while the fresh acid rises to the surface."

Binks (Phil. Mag., s. 3, vol. xi. p. 68) finds, that in dilute sulphuric acid, the size of the copper compared to a given surface of zinc, to produce a maximum effect, should be 16, that of the zinc to a given surface of copper being 7; while in a galvanic arrangement, in which the zinc is immersed in dilute sulphuric acid enclosed in a membranous bag, and the copper in a surrounding solution of sulphate of copper, the proportion of zinc to copper was as one to eight, the impediment to the passage of the current being double in the latter case what it was in the former.*

"*Fifth Experiment.*—Five cylinders of zinc, 10 inches high, $\frac{1}{2}$ diameter, were placed in glass vessels containing sulphuric acid, as before. Into these were placed cylindrical earthenware vessels, $1\frac{1}{2}$ inch diameter, containing pure nitric acid; slips of platina foil were rolled into cylinders as before.

	Cubic inches.
Time, 2 minutes	20·0
After 10	25·0
After 30	30·0†

"From these data may be calculated the heights of the zinc pipes, and the weight of platina foil required to obtain any given decomposition to be employed, as shewn by Jacobi, either as a motive power, or applied to light-houses, to the polariscope, or to the fusion of refractory substances. For the latter purposes I had fixed to a strong shallow woolf-bottle, two tubes with glass cocks, and to them tubes containing chloride of calcium, applied to a Daniell's jet, playing upon a cylinder of lime rotated by clockwork. A third tube was inserted in the bottle, intended as a regulation of the pressure, or a safety-valve, in case of explosion."

Dr. APJOHN then made a brief verbal communication on the subject of the composition of Pyrope. This mineral, long confounded with garnet, is known to be distinguished from it by containing chrome, and by exhibiting, not the dodecahedral, but the hexahedral form. The best analysis of it, however, which are by Kobel and Wachtmiester, are obviously imperfect, of which no better proof can be given than that Gustavus Rose, in his Crystallography, does not attempt to give the formula of the mineral, but contents himself with enumerating the different oxydes of which it is composed. Under these circumstances, Dr. Apjohn conceived that a re-examination of the constitution of pyrope would not be without interest. He there-

* "The experiments of Prof. Daniell have proved, that in the constant battery, when the generating surface formed the circumference of the arrangement, the force was only the half of that evolved when it formed the centre."—Phil. Trans., vol. cxxviii., p. 41.

† "The dilute acid in the voltameter began to boil; the cause of the increase of decomposition, compared to what took place in the small cylinder, was the small stratum of sulphuric acid between the porous vessel and the zinc. For a continuous action, the zinc pipes, sealed at one end and amalgamated, should be connected, by pipes at top and bottom, with an earthenware vessel containing the sulphuric acid."

fore undertook its analysis; and the result has been that he has detected in it yttria, one of the rarest of the earths; one, in fact, which had previously been known to exist only in a few minerals of exceeding scarcity. The yttria was insulated in the following manner:—

The mineral being fused with carbonate of potash, and the silex separated in the usual way, the peroxyde of iron, alumina, and yttria were precipitated together by a mixed solution of ammonia and sal-ammoniac. The alumina was taken up by caustic potash; and to the iron and yttria, dissolved in a minimum of muriatic acid, such a quantity of tartaric acid was added, that upon subsequently pouring in ammonia in excess there was no precipitate produced. The iron was now removed by sulphuretted hydrogen; and the solution evaporated to dryness, and ignited in a large platinum crucible, so as to volatilize the ammoniacal salts and burn away the carbon of the tartaric acid, left the *yttria* slightly coloured by oxyde of chrome. From this latter substance it is separated, but not perfectly, by the action of a dilute acid; and by the addition of ammonia, or caustic potash, to the solution, the yttria is again recovered. That the substance thus obtained is yttria, seems proved by the following considerations:—

It is separated, though not completely, from acids, by ammonia largely diluted with sal-ammoniac, and hence cannot be one of the alkaline earths.

It is insoluble in potash, and is, therefore, not alumina or glucina.

After ignition it dissolves readily in dilute acids, and is hence not zirconia or thorina. From zirconia it is further distinguished by its saline solutions being precipitated by ferrocyanide of potassium.

It is not oxyde of cerium, for it does not redden in the exterior flame of the blow-pipe, and because its salts are not precipitated by the sulphate of potash. The quantity of the yttria amounts to at least 3 per cent.

Dr. Apjohn is still engaged in investigating the composition of pyrope; and expressed his intention of bringing his results on a future occasion in a more detailed form under the notice of the Academy, when he hoped also to be able to assign the true formula of the mineral.

Mr. CLIBBORN made the following communication on the subject of the Leyden Jar:—

“In Brande’s Manual of Chemistry, vol. i., 3rd edition, p. 76, I find it stated, that ‘if one Leyden jar be insulated, with its internal surface connected with the positive conductor, another jar may be charged from its exterior coating; and if this second jar be insulated, a third may be charged from its exterior coating, and so on for any number of jars, provided always that the exterior coating of the last jar be connected with the ground.’

“As my electrifying machine was but small, it occurred to me that I might economise both time and labour by constructing a bat-

tery of jars so arranged that I should be able to take advantage of this principle, and make one jar charge another, instead of my being obliged to charge the whole series; for, though they are all connected together, and charged by the same operation in the common electric battery, yet the time and labour consumed in charging the battery is exactly the same as if each jar were charged separately and then added to the series. A great saving of labour and time would have been effected had the arrangement of jars answered, for it was exactly the same as that described by Brande, so far as the charging part of the apparatus was concerned; but when the jars were loaded, or rather *should have been loaded*, they were made to turn through a quadrant, and form a new arrangement, by which all their outside coatings were connected together by a common conductor. A similar arrangement connected all their inside coatings, which made all the conditions necessary to the perfection of the common battery; and I found it capable of being charged by the electrifying machine in this form; but it could not be charged to any extent in the other. It appeared that but few sparks would pass from the conductor to the first jar. If the last one was removed, and its chain fastened to the next, the first jar would take a few more sparks, and so on; for it was found that whenever the last jar in the series at any time was removed, the same results followed; and this was the case when the last but one was removed, clearly proving that the capacity or aptitude of the first jar to take a charge was influenced and diminished by the second, more so by the third, fourth, &c. Its aptitude was greatest when it was by itself, and not connected, as described, with the others.

"This result disappointed my expectations, so far as my intended improvement on the electric battery was concerned; and it also appeared to point out the existence of a principle influencing the charge of the electric jar, which was not recognized in the popular treatises on electricity. I procured a number of glass plates with fixed and moveable coatings. These plates were insulated and arranged with and without coatings in every way that Brande's rule required, but the general result was the same as that given above.

"From numerous experiments made with these plates, I came to the following general conclusions:—

"1. That the actual quantity of the positive and negative electricities which we can accumulate in the *opposite surfaces* of an electric or non-conductor, as a plate of glass or dry ice, depends upon the *distance* of these surfaces.

"2. Every case of charge of *one jar or plate* may be assimilated to that of any number of jars or plates in a series such as Brande's, by supposing the one jar or plate to be divided into the greater number, its thickness being the *sum of the thicknesses of all the segments or plates*; the inside of the first jar or surface of first plate in contact with the conductor, and outside of last jar or plate in contact with the ground, being considered as the proper *opposite sur-*

faces of the proper plate, and those on which the electricities evolved by the friction of the cylinder and rubber of the electrifying machine are accumulated or heaped.

"If we make a pile of the plates, coated or not, and charge the outside surfaces by coating them, and connecting one with the cylinder and the other with the rubber of the machine, we find all the conditions of the experiment complied with. There is no necessity for any connexion with the ground, which in Brande's can act merely as the conductor to convey the negative charge of the rubber to the extreme surface.

"Let us now unpack the pile, and we find that the charge of the intermediate plates diminishes as we approximate towards the centre of the pile, being greatest near the extremes. At equal distances the charges are equal; for the charges of the first plate but one, and the last but one, will as perfectly neutralize each other as the charges of the surfaces of the first and last. The same is found to be the case with the surfaces of the third plates from each extreme, and so on of the others; but it is not the case with a second and a third, a first and a fourth plate, and so on, *no two unequals as to place* exactly neutralizing each other. Hence we may conclude, that the charge of the intermediate jars in a series such as described by Brande, though it really depends on inductive agency, is altogether different from that kind he alludes to, which may be inferred from his erroneous representation of the actual fact; and the charge of the extreme surfaces is immediately the result of that action only, which several electricians have called conduction, arising from the connexion of these surfaces with the sources of the free electric forces.

"The fact here described appears capable of throwing much light on the corpuscular arrangement of the atoms of bodies which retain an electric charge on their surfaces, or which, by a change of form from mechanical pressure or difference of temperature, exhibit differences of electric state. In speaking of a charged electric, we may consider it a pile of an infinite number of plates, each of which, except the extreme surfaces, is composed of a surface of atoms, which are acted on by two sets of induced electric forces, whose differences, arising from their distances from the extremes, we discover when we split the plate, or if it be a pile, when we separate the plates from each other."

On the Progress effected in the Process of Gilding by the Electro-Chemical Method. By Professor A. DE LA RIVE.*

I shall unite under this title the account given of the progress of this application of science to the arts since I made it known in April, 1840.

Different artists have been occupied on it at Geneva, particularly M. M. Bergem, father and son, who have presented the Academy

* Archives De L'Ectricitie, No. 1.

of Sciences at Paris, in the spring of 1840, objects gilded, and which, after having been submitted to decisive proofs, have been acknowledged as nothing inferior in any respect, either in solidity or brilliance, to the best gilding produced by the mercurial method. These gentlemen have brought to the process which I have described some perfections which they have not made known. This mode of gilding appears to meet with some obstacles in its application to pieces of brass for the movements of clocks or watches, because the colour which it gives them is not such as is usually given to this species of work.

M. Droin, of Geneva, a distinguished workman, who is employed with M. M. Baute and Co., has succeeded, by following to the letter the process which I have described, in producing beautiful specimens of gilding; but he has remarked, that in order to succeed it is necessary to have the metal that we want to gild (silver or brass) perfectly homogeneous, a quality which it is difficult at all times to meet with. The gilding will not be perfectly uniform except under this condition.

M. Perrot, of Rouen, has sent to the Academy of Sciences of Paris many samples of different metals which he has gilded by voltaic currents, and which appear to have succeeded very well.

M. Arago likewise presented the Academy of Sciences, in its session of the 5th of May last, with the spring of a chronometer from the manufactory of M. Dent, gilded to great perfection by means of the galvanic process. He called to mind, on this occasion, that he had presented to the Academy a multitude of objects in metal, gilded by means of the same process by M. Perrot. M. Perrot had already at this time, also, springs to exhibit which were gilded in the same manner, and if they were not comprised in the number of objects which he sent, it was because he was awaiting the completion of an experiment in which he had undertaken to gild, at the same time, all the movements of a watch; to gild them not only whilst in their place, but whilst performing their usual functions.

In Germany, a distinguished artist of Stuttgart, Mr. Reinecker, who is the author of a very remarkable process of waving or watering, has brought the process of gilding by the galvanic method to that point of perfection which, under the relation of solidity, leaves nothing to be desired. As to its beauty, it leaves far in the rear every other species of gilding. "If we consider," adds the *Gazette* of Stuttgart (from whence we draw these details), "that in the process of gilding by galvanism there is no disengagement of those mercurial vapours so hurtful to the operator, that the quantity of gold employed is much less than in the ordinary gilding, and that the greater part of the technical portion of the work is so simple that it may be executed by children, this discovery in this branch of industry will then appear of such importance that we can but express our ardent desire to see it generally employed."

M. Boettiger, in the *Annalen der Chemie und der Pharmacie* (vol. xxxv., p. 350), describes the efforts which he has made to gild and platinize the plates of copper in relief which have been obtained by the process galvanoplastik.

He tried, at first, to platinize them by the galvanic method, making use of a solution of chloride of platina very neuter and very weak. He has succeeded, but he has found that it was necessary to have six immersions in the chloride of platinum to produce a covering of platinum of very small thickness; and again, this covering had not a very good colour, but rather grey than white.

Having found, by succeeding experiments, that for gilding no salt is preferable to the double chloride of gold and sodium, Mr. B. endeavoured to employ, in platinizing, the double chloride of platinum and sodium. The experiments which he has thus made have perfectly succeeded. In most cases he did not find it necessary to give more than three immersions to re-cover with a sufficiently strong layer of platinum the largest surface of copper; the colour also appeared of a much purer white. There is one circumstance, however, to which it is always necessary to pay attention in platinizing copper and other metals, which is, strongly to rub the object with a small piece of linen, and to scour it immediately with chalk each time, without exception, that it is withdrawn from the solution of platinum. It is worthy of remark also, that whether in silvering, in gilding, or in platinizing, those objects which have the highest polish are those also which take the metallic layer with the greatest facility and beauty. Mr. Boettiger thinks, that in order to produce good gilding on copper, it is necessary to commence by covering it with a cuticle of platinum, which is easy, and of very little expense; the gilding is far more beautiful and more durable than that obtained by the direct gilding of the copper. It is easy to obtain the double chloride of platinum and sodium by mixing equal parts of dry chloride of platinum and of pure common salt in distilled water. It is very necessary also to rub the metal conveniently with fine sand, moistened hydrochloric acid mixed with chalk, in such a manner as to leave no traces of the oxyde of copper, for platinum will not be precipitated on those places where any of it remains, and it will suffice to rub again those places, in order to determine this deposit of platinum, when the plate of copper is again put into the solution as the negative metal of the pair.

I cannot avoid remarking, that I have urgently insisted, in my notice, on the necessity of well cleansing the surface of the copper from verdigris before putting them into the solution of gold, and on the importance of successive immersions, and of the rubbing being repeated after each immersion with a fine linen cloth.

M. Snaer, in a work on electro-metalurgy, of which we shall give an account in our next number, employs for gilding voltaic currents far more powerful than those which I have indicated, and

which have been generally used. He is said thus to obtain gilding much more solid, and of any thickness, as great as may be required.

He remarks that the process called the English one, of Elphinstone, does not offer this last advantage. In this process the object to be gilded, which is brass, for example, is put into a solution of double chloride of gold and sodium, raised to high temperature. The gold is precipitated by the effect of the solution of a part of the metal equivalent to that on which the precipitation takes place. Now, as soon as all the surface is re-covered by a thin cuticle of gold, there is no longer a possibility of any part of this surface being dissolved, and thus the gold is no longer precipitated. It can only form, then, a very superficial gilding, and consequently of short duration; whilst by the voltaic process there is no limit to the thickness which may be given to the lamina of gold. Another inconvenience of the English process is, that there ensues, by the dissolution of a part of the surface to be gilded, an alteration of that surface, a circumstance which proves prejudicial in many cases, especially when it acts on objects the dimensions of which have been very exactly calculated, and ought not to be altered, such as the wheels of chronometers, for example.

M. Hammon, an engraver at Geneva, has found great advantage in replacing the engraving by aquafortis, the varnish of which is made to cover the plate of copper by a cuticle of gold precipitated by voltaic agency: the tracings of the needle are far more delicate. The lamina of gold being permanent, and not being broken or carried off like the coating of varnish, when the aquafortis has operated, we may with great facility correct the engraven plate if it has any defect, which is impossible in the old process. We shall give to our readers, in one of our next numbers, a specimen of this species of engraving, by means of which we have produced a new apparatus which we purpose describing on a future occasion.

ROYAL VICTORIA GALLERY, MANCHESTER.

CONVERSAZIONE, FEBRUARY 16, 1841.

On a New Class of Magnetic Forces. By J. P. JOULE, Esq.

As it is my intention to bring forward in this paper an electro-magnetic principle, in reference partly to the motion of machines, I hope that a few preliminary observations with respect to the present class of electro-magnetic engines will not be deemed out of place.

The great attractive powers of the electro-magnet, and the extreme rapidity with which its polarity is reversed by changing the direction of the current, very readily presents to the reflecting mind an idea that its power may be made available for mechanical purposes. Accordingly, as soon as the general principles of electro-magnetism were understood, Professor Henry, Mr. Sturgeon, and

after them a great number of ingenious individuals, constructed various arrangements of machinery to be set in motion by magnetic attraction and repulsion.

At that period the expectations that electro-magnetism would ultimately supersede steam, as a motive force, were very sanguine. There seemed to be nothing to prevent an enormous velocity of rotation, and consequently an enormous power, except the resistance of the air which it was easy to remove, the resistance of iron to the induction of magnetism which I succeeded in overcoming to a great extent by annealing the iron bars very well, and the inertia of the electric fluid.

We are indebted to Professor Jacobi for the exposition of the principal obstacle to the perfection of the electro-magnetic engine. He has shewn that the electric action produced by the motion of the bars operates against the battery current, and in this way reduces the magnetism of the bars, until, at a certain velocity, the forces of attraction become equivalent to the load on the axle, and the motion in consequence ceases to accelerate. The Russian philosopher had not, however, given precise numerical details concerning the duty of his apparatus, nor had he then determined the laws of the engine; I was therefore induced to construct an engine adapted for experimental purposes, in order in some measure to clear up these points.

The experiments which I instituted two years ago with this instrument, were fully detailed in the *Annals of Electricity*, vol. 4, p. 474; I need not, therefore, allude to them in this place further than by stating some of their principal results.

I found, 1st,—That the counter electric action, or, in other words, the magnetic electric resistance to the battery current, is proportional to the velocity of rotation and the magnetism of the bars.

2nd,—That the economical duty at a given velocity of rotation is a constant quantity, whatever the number of similar pairs in series (provided that the resistance of the battery is kept the same).

3rd,—That at small velocities great advantage is obtained by reducing as far as possible the resistance of the battery, and by arranging the coils so as to facilitate as far as possible the transmission of the current.

And 4th,—That the economical duty at a given velocity, and for a given resistance of the battery, is proportional to the mean intensity of the several pairs of the battery.

With my apparatus, every pound of zinc consumed in Grove's battery produced a mechanical force (friction included), equal to raise a weight of 331,400lbs. to the height of one foot, when the revolving magnets were moving at the velocity of eight feet per second.

Now the duty of the best Cornish steam-engines is about 1,500,000lbs. raised to the height of one foot by the combustion of each pound of coal, or nearly five times the extreme duty that I was able to obtain from the magnetic engine by the consumption of

a pound of zinc. This comparison is so very unfavourable, that I must confess that I almost despair of the success of electro-magnetic attractions as an economical source of power; for although my machine was by no means perfect, I do not see how the arrangement of its parts could be improved so far as to make the duty per pound of zinc much superior to the duty of the best steam-engines per lb. of coal. And even if this were attained, the expense of the zinc and exciting fluids of the battery is so great, when compared with the price of coal, as to prevent this class of magnetic engine from being useful for any but very peculiar purposes.

NEW CLASS OF MAGNETIC FORCES.

A few weeks ago an ingenious gentleman, of this town, suggested to me a novel form of electro-magnetic engine. He was of opinion that a bar was increased in length by receiving the magnetic influence; and that, although the increment was perhaps very small, it still might be found valuable as a source of power on account of the great force with which it would operate. At that gentleman's request, I have entered into experiments to ascertain whether his opinion was correct, and if so, into a calculation whether the new source of power might be advantageously adopted for the movement of machinery.

After some preliminary trials, I adopted the following method of experiment. A length of thirty feet of copper wire, one twentieth of an inch thick, and covered with cotton thread, was formed into a coil twenty-two inches long and one-third of an inch in interior diameter. This coil was secured in a perpendicular position, and the rod of iron, of which I wished to ascertain the increment, was suspended in its axis so as to receive the magnetic influence whenever a current of electricity was passed through the coil. Lastly, the upper extremity of the rod was fixed, and the lower extremity was attached to a system of levers which multiplied its motion three thousand times.

A bar of rectangular iron wire, two feet long, one-quarter of an inch broad, and one-eighth of an inch thick, caused the index of the multiplying apparatus to spring from its position, and vibrate about a point, one-tenth of an inch, in advance, when the coil was made to complete the circuit of a battery capable of magnetizing the iron to saturation, or nearly so. After a short interval of time the index ceased to vibrate, and began to advance very gradually in consequence of the expansion of the bar from the heat which was radiated from the coil. On breaking the circuit, the index immediately began to vibrate about a point, exactly one-tenth of an inch, lower than that to which it had attained.

By multiplying the advance of the index by the power of the levers, we have $\frac{1}{30000}$ of an inch, the increment of the bar, which may be otherwise stated as $\frac{1}{720000}$ of its whole length.

Similar results were obtained by the use of an iron wire, two feet long and one-twelfth of an inch in diameter. Five pairs of the nitric acid battery produced an increment of the thirty thousandth part of an inch; and when only one pair of the battery was employed, I had an increment very slightly less, viz., the thirty-three thousandth part of an inch.

This increment does not appear to depend upon the thickness of the bar; for an electro-magnet made of iron, three feet long and one inch square, was found to expand under the magnetic influence to nearly the same extent, compared with its length, as the wires did in the previous experiments.

I made some experiments in order to ascertain the *law* of the increment. Their results proved it to be *very nearly proportional to the intensity of the magnetism and the length of the bar*.

Trial was made whether any effect could be produced by using a *copper* wire, two feet long and about one-tenth of an inch in diameter; but I need scarcely observe that the attempt was unattended with the slightest success.

A very good way of observing the above phenomena is to examine one end of an electro-magnet with a powerful microscope, while the other end is fixed. The increment is then observed to take place with extreme suddenness, as if it had been occasioned by a blow at the other extremity. The expansion, though very minute, is indeed so very rapid, that it may be felt by the touch; and if the electro-magnet be placed perpendicularly on a hard elastic body, such as glass, the ear can readily detect the fact that it makes a slight jump every time that contact is made with the battery.

When one end of the electro-magnet is applied to the ear, a distinct musical sound is heard every time that contact is made with, or broken from, the battery; another proof of the suddenness with which the particles of iron are disturbed.

With regard to the application of this new force to the movement of machinery, I have nothing favourable to advance. An easy calculation on the basis of the modulus of elasticity of iron will shew that an electro-magnet, consisting of a bar of iron one inch square and three feet long, will exert a force of about ten pounds over the space of the twenty thousandth part of an inch, every time that contact with the voltaic battery is made or broken, providing the transmitted current is capable of saturating the iron. If, therefore, contact be made and broken a hundred times per second, for an hour together, we shall have only fifteen pounds raised to the height of one foot. This force is, therefore, far too minute for the movement of machinery; and the duty per pound of zinc is vastly less than that of the common electro-magnetic engine.

We shall now examine the bearing of this new property of the electro-magnet on magnetic theory. We shall consider it in connection with the hypothesis of Ampère and Æpinus.

The theory of Ampère refers the phenomena of magnetism to the

attraction and repulsion of currents of electricity moving in the same or contrary directions. Fig. 1, plate 4, represents the section of six particles of a magnetized bar of iron, according to (perhaps) the best modification of that philosopher's theory. The black circles represent atoms of iron, and the shades around them represent atmospheres of electricity moving in planes at right angles to the axis of the magnet.

This theory affords a good explanation of most cases of magnetic attraction. But the physical conditions which are demanded by it are impossible, and contrary to the analogy of nature, for it must necessarily suppose motion, or at least an active force to be continued against antagonist forces, for an indefinite length of time, without loss, in order to explain the phenomena exhibited by a hard steel magnet.

The only way in which it can account for the fact that iron, after receiving a certain quantity of magnetism, is incapacitated from receiving a further supply, or becomes saturated, is by supposing that the electricity which revolves around each atom of iron has a centrifugal tendency. The velocity of the electric currents around the atoms of iron will tend to be proportional to the inductive influence which urges them, and if the electricity be not endowed with centrifugal force, it is difficult to say why it should refuse to travel beyond a certain velocity; and in that case the phenomenon of saturation is left unexplained. If, however, the momentum of electricity, and its consequent centrifugal tendency when rotated, be supposed to exist, the currents will be prevented from going beyond a certain velocity by their interference with one another.

These principles are, however, less successful in accounting for the increase of length which we have noticed in a bar of iron when under the magnetic influence; for as the electricity is supposed to revolve in a plane at right angles to the axis of the bar, the divergence of the fluid from each atom of iron by centrifugal force would have the effect of *shortening* the mass of iron, which is directly opposed to experience.

If we now turn to a corrected theory* of Æpinus, we shall see that it explains very well, facts which are unaccounted for by the former theory.

Let the black circles in Fig 2 represent six atoms of iron; the shades around them atmospheres of magnetism; and the white rings over these still rarer atmospheres of electricity. Further, let the space between the atoms be supposed to be filled with calorific ether in a state of vibration, or otherwise, to be occupied with the oscillations of the atoms themselves. Such a state of things will probably give a good idea of part of a bar of unmagnetized steel or iron. Now, if an inductive influence be applied to the atoms represented in Fig. 2, the magnetism is supposed to accumu-

* See Dr. Roget's Treatise on Magnetism (s. 133).

late on one side of the atoms of iron, as represented by Fig. 3, and the bar is rendered magnetic.

Such a theory seems to me to afford a natural and complete expression of facts. It supposes nothing which we cannot readily comprehend, except the existence and elementary properties of matter, which are necessarily assumed by every theory, and which the Great Creator has placed utterly beyond the grasp of the human understanding.

When all the magnetism of each atom of iron is accumulated to one side, such atom may be said to be *saturated* with magnetism.

It is obvious, that when the magnetism is accumulating at one side of each particle, the bar will *increase* in length in the direction of its polarity, and *decrease* in a direction at right angles to this. The former fact we have proved by experiment, and there can be little doubt that a very delicate apparatus would exhibit the *diminution* of the *thickness* of a bar of iron in consequence of the communication of magnetic virtue.

Our theory will also account for the fact, that at a certain degree of heat all the magnetic power of iron is destroyed. I have before observed that the space left between the magnetic particles in Fig. 3 represents the room taken up by their vibration. This vibration is called heat, and will, of course, increase in violence and extent with the increase of the temperature of the bar. Now, it is natural to suppose that the atoms of iron have far greater weight and inertia than the atmospheres of magnetism and electricity which surround them; therefore these atmospheres will be in a state of vibration, while the atoms of iron remain in a state of comparative quiescence; and when their vibration has reached a certain extent, the inductive influence will not be able to arrange the magnetism in any definite direction with regard to the atoms of iron.

The retentive power may be explained by supposing the magnetism to adhere to the atoms of iron to a certain extent. And if we make another supposition, viz., that an atom of iron, on combining with an atom of carbon, loses its attraction for magnetism on that side which is next the carbon, the superior retentive power of steel in comparison with that of iron is explained.

Ladies and gentlemen, I have had the honour of laying before you a theory of magnetism, first originated by Æpinus, and somewhat modified by myself. In my opinion it affords a simple and comprehensive expression of facts, which is the chief value of any theory. Whether or not such a state of things as I have assumed really exists in nature, it will require further experiments to demonstrate.

MR. STURGEON'S *Researches in Electricity, Magnetism, &c.*

(Continued from page 88.)

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,—In no experiment hitherto recorded in the whole history of electro-magnetic rotations, do we find employed, at the same time, both poles of the straight bar magnet. Hence all the rotating phænomena yet elicited, by employing that magnet, are only such as obtain by the reciprocal action of one of its poles and a portion of the conducting wire (under various forms) joining the copper and zinc sides of the battery. And when the poles of the magnet, or the direction of the force in the wire is changed, the results of the experiments in every case appear to be reversed. And in explanation, these, as well as every electro-magnetic phænomenon, are generally considered to proceed from a reciprocal disposition of the wire and magnet to place themselves at right angles to each other, for, when so situated, and at, or near, the centre of the latter, nothing further can be obtained; for both wire and magnet, when free to move, will, in this position, remain at rest.

Convinced as it were that the rotations are in different directions on the opposite poles when employed singly, it would seem as if experimenters had been fully persuaded of the impossibility of obtaining rotations by bringing into action, at the same time, both poles of the above-named magnet; or, whatever else may have prevented them, we have never yet heard of anything of the kind being attempted.

By referring the reader to a postscript in my last paper on thermo-rotations, and which appeared in your journal for April last, it will be there seen that a rotation had been obtained under the above circumstances; and that I had at that time rotated the wire or system of wires in the form of a sphere around, and by the influence of, both poles of an internal magnet.*

This experiment, it was observed, first suggested itself on reading Dr. Halley's theory of terrestrial magnetism, and was instituted with a view not only to be somewhat imitative of that philosopher's hypothesis, but likewise to serve in part to exhibit the earth's rotative motion on its axis.

It is by no means intended from this experiment to assert, that the rotatory motion of the earth and planets is really the effect of electro-magnetism, or that the variation of the compass-needle depends upon the theory alluded to; but merely to detail an experimental fact as exhibited by the apparatus in its present imperfect state, the mechanism of which confines the experiment greatly within the limits it might have been extended to. However, it already proves that a galvanized sphere, when free to move and containing within it a magnetic nucleus or kernel, *will rotate* by the influence of that central magnet; and that also, were the magnet free to move,

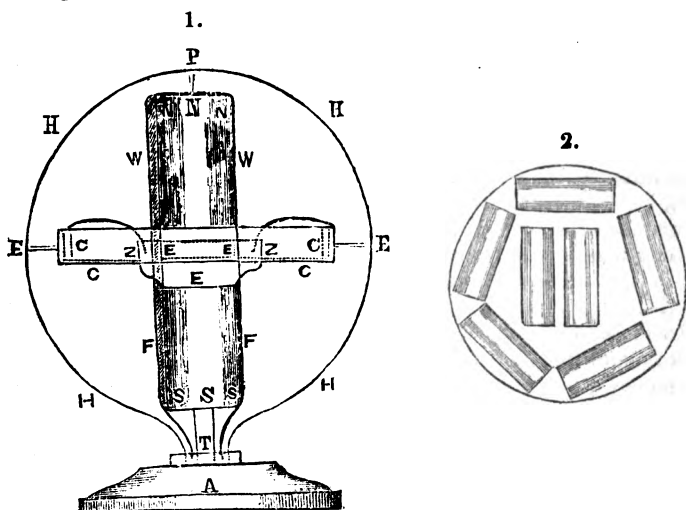
* See page 88 of this volume.

a probability is manifested that it would rotate at the same time, *i. e.* they would both rotate at the same time by the reciprocal action of each other.

Having during my leisure hours been busily employed in fitting up an apparatus for other new experiments, I have not, till now, had an opportunity to describe that by which this is made. The experiment, however, has been witnessed during that time by gentlemen eminent in this branch of philosophy, and who have uniformly acknowledged it to be an imposing and pleasing spectacle. It is now offered, with some others which it is presumed are the first of the kind, and which possibly may not be thought uninteresting to some of your readers.

Description of the Apparatus.

Fig. 1 is an elevation of the apparatus; nNn , sSs , a brass cylindrical tube containing seven bar magnets, each of which is eight inches long. These magnets are placed in a frame in the lower part of the cylinder, the upper end of which, being moveable, can be taken off at any time to change their position for the purpose of varying the experiment. Fig. 2 is a transverse section of the cylinder with its magnets.



As the like poles of the magnets are all placed in the same end of the tube when used, the latter, with its contents, may be considered as a compound cylindrical magnet. At the lower end S of the cylinder is a stout brass stud T, which stands firm in a socket on the top of the foot A. Around this socket is a cell for the purpose of holding quicksilver. Another cell *eee* fits gently on the outside of the cylinder, and is suspended over its top part by a stout brass wire

ww, the extremities of which communicate with the mercury in the cell *eee*. Another connecting wire *ff* communicates with the mercury in both cells. *Cc Cc* is a copper vessel made in the form of an Ampère's rotating cylinder, seven inches diameter; in this vessel is placed a cylinder of zinc, as represented by the dotted lines *zz*. On the outer rim, and at 90° from each other, are soldered four brass studs; to the extremities of which are soldered two brass wire circles (one only of which is shewn in the figure) that cross each other at right angles in the pole *P*; from which junction descends a pivot that runs in a hole made to receive it in the upper part of the connecting wire *ww*. The two circles are each $9\frac{1}{2}$ inches diameter, and form two great circles of the sphere they are intended to represent. The lower parts of the circles are left open, and the points of the wires bent downwards, till they come in contact with the mercury in the lower cell. Two other wires proceed from the upper edge of the zinc cylinder, and terminate in the mercury in the upper cell *eee*, as shewn in the figure.

Things being thus arranged, when dilute acid is poured into the copper vessel, the galvanic action takes place throughout every part of the great circles which represent the sphere; ascending from the copper in the upper hemisphere *HH* from the equator *EE* to the pole *P*; and descending in the lower hemisphere *hh* from the equator to the mercury in the lower cell, which is the place of the other pole of the sphere. From the poles it is transmitted to the mercury in the upper cell *eee*, by the conducting wires *ww* and *ff*; and both circuits are completed by the wires from the zinc cylinder terminating in this cell. Thus two galvanic circuits are formed by one apparatus; and hence likewise is solved the apparent anomalous phenomena of galvanized wires or cylinders rotating in different directions on the opposite poles of a magnet. This delusive appearance is completely *relative*, and similar to our notions of *up and down*; but with respect to the magnet, and in reality, the rotation is the same on both poles: for example, let any person turn first towards the north, and then towards the south; by observing the heavenly bodies, they would appear to revolve to the left in the former, and to the right in the latter case; yet the rotative motion of the earth (which is the cause of their apparent motion) is constantly the same way. In this experiment both poles of the magnet are acted on at the same time by similar galvanized wires, and the machine rotates by the joint influence of the former on the excited wires of the latter. Owing, however, to the weight and size of the machine, the rotation is but slow at the beginning of the experiment, but soon acquires considerable velocity, and the generating force, added to the acquired momentum, keeps it in motion for a length of time.

Well aware of the difficulties that would arise at the present day in any endeavour to support an hypothesis of the earth's being hollow, and containing within it a spherical magnet, as was the opinion of Dr. Halley, I have confined myself to a detail of the experi-

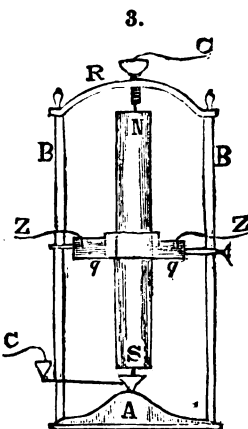
ment only, without obtruding either remark or opinion. The success of this experiment, however, so satisfactorily confirmed my expectations, that the principle upon which it was made could hardly fail to intimate its applicability to others of a like nature; one of which being intimately connected with that I have just described, this may, perhaps, be considered no improper place for its detail.

The experiment for rotating the magnet on its axis, it is well known, was first made by M. Ampère; but the manner in which as yet it has been exhibited, like all the other similar experiments, shews the action only on one-half of the magnet at one time;—and by reversing the poles (everything else remaining as before) it appears to rotate in a contrary direction.

I hope, however, that I have satisfactorily proved that this apparent contrariety of rotation is merely relative, and that the real motion is the same in both cases. If possible, however, the experiment I am about to describe will confirm this property in a still more satisfactory manner.

The rotation of the magnet on its axis by subjecting both poles at the same time to the influence of similar electric currents.

Fig. 3 is an elevation of the apparatus; and, with the exception of the magnet NS, is entirely of brass-work. A is a round foot, from which rise the two cheeks BB, for the purpose of supporting the annular cell *qq* holding quicksilver; and likewise the cross piece R, which is screwed tight down by the knobs *k k*, and thus the apparatus is kept firm and steady. The magnet passes through the opening in the centre of the cell *qq*, and communicates with the mercury there contained by means of a wire soldered to its equator, and at right angles to its axis. The poles are furnished with pivots; the lower of which at S runs in a small cup on the top of the foot A; and the upper one at N runs in the lower extremity of a screw nail passing through the cross piece R, and whose head is made into the form of a cup for holding mercury. Z C are two wires proceeding respectively from the zinc and copper side of a battery: the former communicating with the mercury in the cell *qq*, and consequently with the equator of the magnet; the latter communicates with the upper pole N in the manner shewn in the figure. Thus, one-half of the magnet forms part of the galvanic circuit. The other half, from its equator to the lower pole S, forms likewise part of another similar galvanic circuit, by means of the wires Z' C' which proceed respec-



tively from the zinc and copper sides of another battery, and communicate with the equator and pole in the same manner as the former. By subjecting the magnet in this manner to the influence of like electric currents, it is rotated with an astonishing velocity, but which may in a moment be retarded by interrupting the circuit of either battery; and again accelerated by renewing the contact. This variation of the experiment proves to demonstration the utility of employing both poles at the same time, and is another proof of the rotations on opposite poles, in the old experiments, being merely relative; for the poles of the magnet are here both connected with the copper side, and the equator with the zinc side of the respective batteries. Another advantage in the manner of making this experiment is, that the glass vessel and mercury for floating the magnet are here not used; therefore the whole of the magnet is in view; whereas in the old mode, only a small portion of the magnet, about the thickness of a quill, was visible above the surface of the mercury. It can be no small gratification to those who are in the habit of giving public lectures, to be enabled to exhibit this experiment to the satisfaction of a large audience; for as the lecturer can now have his rotating magnet of almost any size he pleases, and likewise of any figure, this interesting experiment may be viewed from the remotest part of the lecture-room. Another inconvenience I have almost entirely removed both in making this and every other electro-magnetic experiment. My batteries are of such a peculiar construction as not to annoy the experimenter by the escape of hydrogen; neither is the expense of making the experiments more than one-fortieth of any other method yet made public; yet the apparatus in general is of large dimensions; for instance, the sphere in the former experiment is $9\frac{1}{2}$ inches diameter; and the magnet in the latter is 8 inches long.

I should now proceed to the description of other new experiments, were I not confident that I have already intruded upon your valuable pages. That task must therefore be deferred for the present. Some of the minor of those experiments are,—The rotation of the cylinder by the influence of an external magnet;—Ampère's cylinders rotated independent of each other's weight;—Thermo-rotation on both poles of the magnet;—Electro-magnetic bells, &c.

In summing up the results of the two detailed experiments, it appears from the first,—

That similar electrized wires rotate in the same direction round both poles of the magnet;

That both the copper and zinc cylinders are here carried round the magnet in the same direction;

That a sphere, conducting similar currents of electricity from its equator to its poles, will rotate by the influence of an internal magnet.

And from the second it is evident that, had the magnet in the first experiment been free to move, it would likewise have rotated

at the same time with the sphere containing it. This property would seem somewhat conformable to the opinion of Halley, who supposed the earth to contain a spherical magnet, which rotated within the shell that we inhabit. And, what is more fortunate to the analogy, it is proved by the experiments that when the electric currents are of the same kind from the equator to the poles of both nucleus and shell, they both rotate in the same direction. The rotation would likewise be as effectual, were the magnetic poles removed to some distance from the axis of motion.

Another hypothesis might be advanced to account for the rotatory motion of the earth, and which would not require the supposition of its being hollow; but only to be regarded as a grand natural magnet, possessing a capacity for conducting the electric fluid. The former of those properties is admitted by almost every writer on magnetics; and the latter, it is presumed, will be as readily conceded.

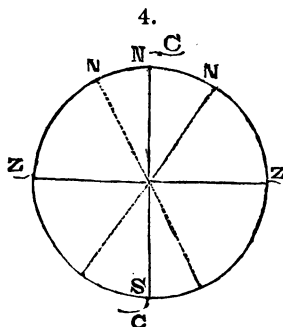
From the second experiment we see the magnet rotate on its axis by the influence of electric currents from its equator to its poles. Had the magnet been a sphere instead of a bar, it might have represented the earth or planet more perfectly; but its figure, it is presumed, can make no difference in the result of the experiment.

It is now well known that electricity can be excited by heat, and the success of thermo rotations in the manner that I obtain them would, if formed into a sphere, depend upon the difference of temperature between the equator and poles. This property is obviously analogous to the natural state of the earth; for the sun exerting his greatest influence a few degrees only on each side of the equator, the polar regions are constantly kept at a very low temperature; so much so, that every attempt yet made to explore them has been rendered abortive in consequence of a prevailing intensity of cold; whilst the equatorial parts, it is well known, are as constantly kept in the other extreme. That thunder and lightning are by far more prevalent in the torrid than in the frigid zones, is a fact that cannot be denied; and that this difference of electrical phænomena is principally caused by the superior action of the sun in those parts, appears more than probable. Hence it may be fairly concluded, that the action of the sun either partly or wholly governs the general electrical phænomena of nature; and, either by producing or exciting this wonderful agency in the equatorial regions, dispenses its influence from thence to the poles of the earth.

Another circumstance that would be something in favour of the hypothesis is, that a magnetic body free to move in any direction, (a sphere suspended in space, for instance,) and having currents of electricity passing over its surface to its poles, would not only rotate, but would likewise maintain its parallelism. This may be easily demonstrated upon the principles shewn in Professor Barlow's

"Magnetic Attractions," second edition, p. 249, under the head Electro-magnetism.

Let $NZC'Z'$, fig. 4, represent the magnetic sphere. NS its poles. Likewise suppose ZC and $Z'C$, to proceed from the zinc and copper sides of two batteries of equal power. Then the electric force in that part of the magnet between Z and C , or in the northern hemisphere, will tend to carry the pole N towards N . And the electric force in the southern hemisphere between Z' and C' will at the same time have a like tendency to carry the same pole N towards N' . But the pole N being acted on by the two equal and contrary forces, can have no tendency to either of these points; therefore must of necessity remain in its original position.



That pole of the magnet which possesses the same kind of magnetism as the north magnetic pole of our globe, being now properly called *north*; when the zinc sides of the batteries are applied at the equator, the magnet rotates from *west* to *east*, or, in the same direction as the earth.

Having pointed out these particulars, most of which are facts deducible from experiment, there can appear no extreme improbability that most of the phænomena which are observed to obtain with the heavenly bodies in our system, are physically produced by the powerful agency of electricity. And experiments may, in my humble opinion, be instituted, that would satisfactorily exhibit the whole; for we have already seen that rotation and parallelism are producible by electro-magnetism; and the thermo experiments serve to prove that the influence of the sun may be sufficient to excite continual electricity. Hence magnetic bodies placed within its influence may not only rotate and keep their axes parallel, but probably may likewise obey every other observed astronomical law with regular mathematical precision.

I am, gentlemen,

Your most obedient servant,

Artillery Place, Woolwich, Aug., 1824.

WM. STURGEON.

(To be continued.)

NOTICES OF NEW BOOKS, &c.

The Eighth Annual Report of the Royal Cornwall Polytechnic Society. SIMPKIN and MARSHALL, London.

The volume before us is another indication of the advancing usefulness of this highly valuable Society, and of the great importance to the advancement of the arts, manufactures, &c. of this country,

which such fostering and stimulating institutions would lead to, were they established and similarly conducted in our busy manufacturing districts, where competition, at present, is so actively carried on with a view to *undersell*, by the production of *deceptive* rather than by *really improved* articles of manufacture; and where genius is suffered to pine, and even smother, in obscurity, for want of aid, or basely robbed of its treasures for want of protection.

The premiums, prizes, &c., awarded to successful candidates, at the eighth annual meeting of the Royal Cornwall Polytechnic Society, were as follow:—Fifteen premiums, in small sums, amounting to upwards of forty pounds; four silver medals; seventeen bronze medals, and about forty-six other prizes of various kinds. By these means the Society at this meeting has acknowledged the talents of, and conferred its honours upon, upwards of fourscore persons; who are thus stimulated to future exertions in the most laudable pursuits to which the human mind can be directed.

The contributions of papers, essays, descriptions of inventions, &c., that were read at the eighth annual meeting, are on the following subjects:—

1st.—*Observations in Illustration of the History and Statistics of the Pilchard Fishery.* By JONATHAN COUCH, Esq. F.L.S.—This paper is mostly confined to a narration of certain purchases and sales of salt and pilchards, by “Richard Trevill, an eminent merchant in the reign of Queen Elizabeth;” which is, no doubt, interesting in the locality of the Pilchard Fisheries.

2.—*Statistics of the Pilchard Fishery, for the years 1838 and 1839.* By Mr. J. S. COURTENAY.

3.—*Comparative Longevity of Miners.* By Mr. ROBERT BLEE, jun.—It appears, from this document, that the average age at which miners die is different in different mines. In the Gwennap mine the average age is 46; in the Illogan, 49; and in the Camborne, 54. The average deaths from mine accidents has been 17 per cent. for the three previous years.

4.—*Statistics of the Falmouth Public Dispensary and Humane Society.*—This is a table of items, by LOVEL SQUIRE.

5.—*On the Atmosphere of Mines.* By M. P. MOYLE, Esq.—This is a valuable scientific document, being a detail of results of chemical analysis of the air in different mines. For the particulars of these experiments we must refer the reader to the work itself: it will be sufficient, in this place, to shew the centage of oxygen in different trials, and under different circumstances:—

At WHEAL VOR.—A tin mine in slate. At 280 fathoms depth the per cent. of oxygen was 18·416; at 250 fathoms, oxygen 16·69 per cent.; at 230 fathoms, oxygen 17·282 per cent.; at 214 fathoms, oxygen 16·762 per cent.; at 36 fathoms, oxygen 14·76 per cent.

GREAT WORK.—Tin and copper mine; granite. At 170 fathoms, oxygen 17·544 per cent.

BINNER DOWNS.—Copper mine. At 104 fathoms from surface, oxygen 16·764 per cent.

CARN BREA.—Copper mine; granite. At 105 fathoms, oxygen 16·693 per cent.; at 95 fathoms, oxygen 14·51 per cent.

TRESAVEAN.—Copper mine in granite. At 156 fathoms, oxygen 16·35 per cent.

WEAL ANN.—Tin mine in granite. At 80 fathoms, oxygen 16·72 per cent.; at 100 fathoms, oxygen 18·22 per cent.

CONSOLS.—Slate. At the bottom levels (no depth given), oxygen 17·78 per cent; second sample of air, oxygen 18·42 per cent.

Mr. Moyle concludes with the following appropriate observation: "The superabundant quantity of nitrogen and carbonic acid above that of a pure atmosphere, clearly points to a source of great mischief to the animal economy; and when combined with the great deficiency of vital air, or oxygen, we cannot much wonder at finding the predominance of pulmonary affections amongst our miners."

6.—*On Asphyxia. Condensed from a Communication by M. P. MOYLE, Esq.*—As this epitome may be interesting to many of our readers, we give it complete:—

"Asphyxy may be defined to be suspended animation proceeding from a primary arrest of the respiratory actions, the other functions being thereby abolished.

"Asphyxy has been very generally viewed as proceeding from causes which act in various ways; and the opinions respecting their nature and mode of operation have been extremely deficient in precision and accuracy. According to the meaning of the term above, asphyxy can only occur in a direct or primary manner, and from causes which either exclude the air from the lungs, prevent its renewal, or abstract that constituent of it which is requisite to the respiratory functions.

"Though submersion be the most frequent, it is by no means the only cause of apparent death worthy the inquiry of the physiologist; but the suspension of vital action from strangulation and noxious gases exhibits phenomena so nearly similar, and require a treatment so strictly congenial, that any enquiry into the nature of submersion only would be narrow and incomplete. I shall therefore attempt a short description of the phenomena of departing life from the foregoing causes; an investigation of the efficacy of the means of cure, sanctioned by experience; and lastly, draw up a few short directions, which might be generally adopted by all those who might first gain access to the unfortunate individual who may be the subject of asphyxy.

"The *duration of life* in cases of asphyxy is very different. In general, the more slowly the abolition of the respiratory function takes place, as in drowning, the longer does the action of the heart continue, although feebly and slowly, even after respiration has ceased; and to this circumstance, as well as to the fluidity of the blood, which is long preserved, is owing the power we possess

of recalling the asphyxied to life; the more *slowly* the state of asphyxy supervenes, the longer the person retains the ability of being re-animated, and *vice versa*.

"A minute anatomical description of the heart and lungs, the theory of respiration, or the appearances on dissection, do not become necessary in the present communication. My principal object being to draw a more general and just consideration of the importance of possessing sufficient medical knowledge as to be capable of rendering immediate assistance of the proper description, by the timely and judicious application of which, the lives of many of our fellow creatures may be saved, I shall therefore, in the first instance, give a line of general treatment to be adopted, and conclude with a few brief remarks for the guidance of all classes of persons, and whereby they may act with full confidence of doing their best for the patient until the arrival of superior aid and judgment.

General Treatment.

"The indications which naturally suggest themselves from the consideration of the causes of asphyxy, their mode of operation, and the ultimate results which they produce, are, 1st, to remove the patient as soon as possible from the causes which occasioned the asphyxied state; and 2nd, to restore the function of respiration, and through it, the circulation.

"The restoration of the function of respiration is to be attempted by various means, calculated, in the first place, to dislodge the impure air contained in the lungs; secondly, to replace it with pure air; thirdly, to excite the remaining vitality of the nerves and muscles; and fourthly, to restore the circulation by means calculated to return the blood from the lungs to the left side of the heart. The simultaneous attainment, as far as may be, of these objects, is to be attempted by a judicious combination of means.

"The patient should be placed on his back, in the open air of a mild and somewhat warm temperature, with the chest, shoulders, and head slightly elevated. He should be stripped of his clothing, and enveloped in a warm blanket; pressure should be made upon the breast and abdomen, alternating with relaxation, in such a manner as to simulate the action of the chest in respiration. By this means the foul air will be thrown out of the lungs; and the restoration of the capacity of the thorax, upon the removal of the momentary pressure, by the elasticity of the costal cartilages, will draw fresh air into the lungs. It will sometimes be of service to apply a hand upon each side of the thorax below the arm-pits, and by gentle shocks endeavour to expel the vitiated air. Whilst this is being performed, bottles of warm water should be applied to the feet, under the knee joints, on the inside of the thighs, and under the arm-pits. Dry warmth is particularly beneficial when applied to the epigastric region. After having used pressure, so as to simulate respiration for a few moments, insufflation of the lungs

must be had recourse to, either by the mouth of the operator, or by a pair of bellows. The patient's nostrils must be closed, while the operator blows with considerable force at once into the chest; then press the chest as before directed, and so repeat the operations. If the lungs can be well inflated by these means, we must persevere for a considerable time; but if it fails to produce the necessary inflation, close the mouth and one nostril, while the operator blows forcibly into the other, to produce the wished-for effect. A small tube of wood, metal, or a tobacco pipe, if at hand, would be a very serviceable instrument for this purpose, but if not at hand, a moment must not be lost in accomplishing it. Insufflation by the breath of another, although not of so pure an air, yet it more than counterbalances this defect by its superior temperature. This inflation and expellation of air should be repeated about fifteen times in a minute. Whilst the insufflation is going on, frictions over various parts of the body, particularly the chest, must be conducted by others, and where electricity or galvanism can be had recourse to, some powerful shocks might now with advantage be passed through the heart and lungs. The nostrils might likewise be irritated with a feather dipped in hartshorn; and some warm, stimulating liquids, as spirits and water, be conveyed, by means of an elastic tube into the stomach, or by means of a glyster into the bowels, which frequently proves of great service. Bleeding should seldom be had recourse to; and it appears not to be warranted except the countenance be swollen, injected, or purplish, and the veins full and distinct, which is often the case after hanging, but seldom from immersion or suffocation. Bleeding may be of much greater benefit in the succeeding stages after we have restored animation.

"The foregoing remedies should be persisted in for some hours, and we should not be satisfied that life is *extinct* because our efforts are not *speedily* crowned with success; nothing should convince us of the futility of the means adopted, unless stiffness of the limbs and other indications of death present themselves.

"I now proceed to those short directions which I could wish to see placed in the hands of all sailors, miners, and inhabitants of sea-ports, in fact, universally distributed; it might then prove the happy means, under Divine Providence, of rescuing more than one unfortunate fellow creature from a premature death.

Drowning.

"Suppose we witness the upsetting of a boat or other accident, whereby one or more are submerged:—Instantly dispatch a messenger for medical aid, while we ourselves hasten to the spot;—remove as quickly as possible all the wet clothes from the body, and supply their place by some of our own;—raise the head and shoulders, while you convey it to the nearest warmth and shelter, and while warm blankets, beds, &c., are in progress, simulate the act of respiration, by first inflating the lungs by blowing strongly with your own mouth through the mouth or nostrils of the patient;

then as quickly press both sides of the chest with your hands, and so on alternately about fifteen times in a minute; and when this has been repeated a few times, let others rub different parts of the body with warm hands, or flannels smeared with butter or oil, while the process of inflation, &c., is still persevered with. Let no salt or spirits be rubbed over the body, as the cold thereby produced is prejudicial.

"The body may be placed before a good fire, or in a warm bath of the temperature of about 100° , or it may be placed in bed between warm blankets or two warm persons, while you persevere in the above mode of imitating natural respiration, until the arrival of superior aid; the restoration of the circulation, and in all probability the life of your friend or neighbour, mainly depends on your early and correct performance of this operation.

Hanging.

"In cases of hanging, as in all other accidents, dispatch a messenger for a medical man, and direct your first efforts towards the stricture on the windpipe caused by the cord; then proceed, as directed in cases of drowning, to inflate the lungs by blowing forcibly into the mouth, press out the air which you have forced in, by your hands on either side of the chest, and so continue the operation. In case there should be a *palleness* of countenance, together with coldness of the body and limbs, you might use friction as before directed; but should the face be flushed, you had better defer the use of frictions until blood is removed from the jugular vein.

Noxious Gases.

"When suspended respiration arises from a person descending incautiously into an old well, mine, brewers' vat, or by sleeping on a lime kiln, or in a room with burning charcoal, &c., we immediately conclude it arises from the presence of carbonic acid. If in a confined pit, first dash a few bucketsful of water into the place, and direct some quicklime to be quenched as near the body as possible; if a candle will continue lighting near the body you may safely venture down for its removal, (and which should be as quickly performed as possible) to the pure air. Similar steps should be adopted in a mine where the suffocation appears to arise from gunpowder smoke. As soon as you reach the body, blow forcibly into his mouth with your own, taking care to close his nostrils, then press the chest as before directed, and continue to repeat it by every possible means. When brought into the pure air, similar means must be adopted as directed under the head of drowning; but we must remember that in every case of apparent death from these causes, our first efforts must be to inflate the lungs with the warmest and purest air possible, then expel it by pressing the chest with both of the hands, then inflate again, and again expel it, and so on. You had better continue this operation without interruption until the arrival of medical aid, than venture on any other mode, the operation of which you may not be fully competent to judge, and risk the

annihilation of a remaining spark of life, by an injudicious use of it."

7.—*On a Mercury Safety Valve.* By Mr. OCTAVIUS WILLIAMS.

8.—*On a Self-regulating Feeding Apparatus, an Alarum and Water Indicator for Steam Boilers.* By Mr. OCTAVIUS WILLIAMS.

9.—*On a Method of Patching Steam Boilers, without lifting out all the water.* By Mr. JOHN HOLMAN.

10.—*Description of the Model of a Fishing Boat, fitted so as to prevent her sinking in the event of her filling with water.* By Mr. J. HUSBANDS.

11.—*Description of Mr. J. PHILLIPS's Tide Wheel.*

12.—*On the Machinery used for raising Miners in the Hartz.* By JOHN BASSET, Esq. M.P.

13.—*Description and Account of a New Kind of Mariner's Compass.* By W. SNOW HARRIS, Esq. F.R.S., &c. &c.

"The compass needle consists of a straight bar about 7 inches long, $\frac{1}{2}$ an inch wide, and $\frac{1}{8}$ of an inch thick; it is made of fine steel, well tempered and hardened throughout its whole length; it is placed edgeways, and is delicately balanced on a fine point, resting on a centre of agate. Instead of the ordinary compass card, a transparent circular disc of talc is attached to the bar, at its under edge, on which the different points, &c., are either painted in transparent colours, or otherwise laid on it, on very thin paper, so that the whole is quite transparent; and in order to ensure and regulate the horizontal position of the whole, at any time or place, there are two small sliders of brass underneath, and on each side of the centre, so contrived as to slide and turn with friction into any required direction by means of two slits in them and small stop screws.

"The compass bar, with its talc circle beneath, is now placed centrally within a ring of hammered or rolled copper, the poles of the bar, which project a little beyond the card, being distant from the interior of the ring about $\frac{1}{8}$ th or $\frac{1}{4}$ th of an inch. This copper ring is about $1\frac{1}{4}$ inch wide, and $\frac{3}{8}$ th thick; it is turned up and finished in a lathe, so as to be perfectly circular; the centre piece carrying the needle is supported on a cross bar attached to the ring, and the centre part accurately adjusted in the lathe.

"The whole is finally set within a glass bowl or other case, according as it is required to light the compass from beneath or above, and placed in gimbles in the usual way,—the perfect transparency of the single disc of talc renders the compass card very visible and clear when lighted from beneath.

"The great steadiness of the compass under all sorts of motion is very remarkable. It has been successfully tried in a few ships of the navy, and is still on trial. The needle being placed on its edge, it is liable to little error in its magnetic line; it has, besides, great magnetic energy and great delicacy of suspension, and is unembarrassed by a heavy card—whilst at the same time the

magneto-electrical induction on the copper ring effectually preserves its natural direction undisturbed—these are advantages of no ordinary kind.

“Mr. Harris has shewn (Transactions of the Royal Society for 1831), that when a magnetic bar oscillates freely within a series of concentric rings of copper, accurately and closely fitted one within the other, the restraining force of the copper with a given magnet is inversely as the squares of the distances from the pole of the bar, and directly as the quantity of copper within the sphere of its action, the matter being supposed to be condensed into an indefinitely thin ring, and taken at some intermediate or mean distance within the surface, where the sum of the forces may be supposed to produce the same effect as if exerted from every part of the mass, and that hence the energy is also directly as the density. He also found that with a given magnetic tension in the bar, the restraining force no longer increased sensibly after a certain number of rings; that in fact the number of rings requisite to exhaust as it were the magnetic force, varied in some ratio of the power of the magnet; thus, with the bar employed, no sensible increase of energy in the whole was observable, after the tenth ring, the effect being the same with ten as with any greater number of rings.

“In the application, therefore, of a copper ring to the purpose of restraining the oscillations of the compass at sea, it is desirable to have the poles of the bar as near as we can to the interior of the ring, to have the copper as dense as possible, and give it greater or less thickness, in proportion to the power of the compass bar. Much has been said, and many experiments tried, with a view of determining the best form for a compass needle; it will, however, probably be found, that the simple bar above described is, upon the whole, not only the most accurate, but in every respect the best. Its form greatly simplifies the workmanship necessary to its construction, and admits of the various other scientific processes upon which its action depends being easily and perfectly carried out. If the steel be well chosen, and be properly tempered, such a bar is susceptible of a very high degree of magnetic energy, which it will be found to completely retain.”

Remarks.

We shall at all times be glad to hear of improvements in so valuable an instrument as the steering compass; but the *operation* of every novel principle, attempted to be introduced, ought to be clearly and minutely explained, and most scrupulously examined, before its adoption can be warranted and justified by science. In the present case, such inquiries as the following are of great importance, a direct and unequivocal answer to which being the only means of ascertaining whether the newly-introduced principle be an improvement, or otherwise, to the steering compass.

What are the respective *routes* of those electric currents which are produced by the needle's *eastward*, *westward*, and *vertical* motions?

What are the arrangements of the *north* and *south* magnetic forces of those currents respectively?

In what manner does each individual magnetic arrangement operate on the needle?

As the Admiralty, and indeed every naval officer in the service, must be deeply interested in this subject, Mr. Harris can have no objections to giving them that satisfaction which they have a right to demand on the above-named particulars, which require full, clear, and immediate explanation.

Should *steadiness* be an essential qualification to constitute a good practical steering compass, that point would be easily attained by vanes of any light material attached at right angles to the under side of card.

14.—*Meteorological Register, Map and Tables for Barometer, Thermometer, and Rain-gauge, at various places.*

15.—*Remarks on Electro-Metallurgy.* By Mr. T. B. JORDAN.

“In the course of a limited series of experiments, which I have tried in this interesting branch of the useful application of electricity, a few ideas have occurred to me, which, I believe, possess the charm of novelty. As no suggestion, however trivial in itself, can be considered altogether unimportant, when it bears on the practical application of science to our manufacturing arts, I feel no hesitation in laying before the Society the following remarks, with a view to their being made public. It is quite evident that Mr. Spencer’s recent discovery of the art of depositing metals from a solution of their salts by galvanic action, has placed in the hands of manufacturers a new means of copying, and the importance of this will be immediately apparent to those who know how large a portion of our manufactures in metal are the copies of original matrixes, or more indirectly the results of other copying processes. Some imagine that because it is only the means of copying, it is really of little value; but it is impossible to allow this to be a valid objection, when we consider how very few metal articles there are, which do not wholly or in part owe their origin to copying. If, then, it can be shewn that this method possesses advantages for some description of work, which are not common to any other, and that there is good reason to suppose it may be economically conducted, then we may fairly infer, that it will in due time become one of the ordinary processes of our factories.

“Its advantages are numerous, and some of them so self evident that they only require naming to be allowed; for instance, any surface, whatever be its material or form, may be formed with a coating of metal, without heat and without force; this coating may be allowed to accumulate to the required thickness, and may then be removed from its original, when the surfaces which were in contact will be found to be so perfectly alike, that it is impossible to discover the minutest scratch in one which has not its counterpart in the other. In proof of the accuracy of the copy, I

may refer to the numerous admirable copies of engraved plates and medals already before the public : if any of these specimens be examined with a glass, it will be found that not only every line of the graver, but every scratch in the polished surface of the original plate, is faithfully transferred, both to the deposited matrix and the subsequent copy of it; and when we consider the fact that both these copies are of the same material as the original plate, and that that may just as well have been of a much softer and more workable material, we must immediately conclude that there is no other known mode of copying possessing like advantages. But as the application of it to the coping of engravings, medals, wood-cuts, and similar subjects, is already well known, I merely refer to it in illustration of my views, and will now offer a few reasons for supposing that it may eventually be found an economical process for some of the more ordinary branches of our manufactures in metal.

"I can now only explain the principles which lead me to imagine that it would not be a costly process on the large scale. In my preliminary experiments, I have used Mr. Smee's battery of platinated silver and zinc; excited by diluted sulphuric acid, it acts well and is more convenient than the sulphate of copper batteries.

"Mr. Spencer's very simple and cheap arrangements, in which the negative plate of a single pair is that receiving the deposit, is, I think, very suitable for flat work; but for complicated forms, I should certainly prefer having the battery arrangement separate from the depositing cell, both on account of its being more manageable, and because in this way the solution of the sulphate of copper is kept constantly at the point of saturation by the destruction of a portion of the copper plate connected with the negative plate of the battery. Of course the solution of this plate is effected by the portion of acid set free by depositing its copper on the mould, assisted by the galvanic action, so that in fact this method gives us the power of removing the material from any old and irregular piece of metal, and depositing it in the most highly wrought mould of which we may wish to obtain a copy.

In taking a comparative view of the probable cost of production, it will not be necessary to consider the first outlay for apparatus and moulds, because there must be a much larger outlay before commencing operations in the usual way; and leaving this out of the question, the cost of any electro-deposit would be made up of the value of the raw material, of the zinc and acid used in the battery, and of the time employed in making the arrangements.

"The raw material may be obtained in considerable quantities, at its lowest price, as old sheathing copper would be admirably adapted to the purpose, on account of the facility with which it may be bent to any required form. The zinc and acid used in the battery would only have combined to form sulphate of zinc, which would be of some value. The time required for making the arrangements in the different troughs of a large factory would be

comparatively small, because one man may keep a great number at work, as they only require attention once in twenty-four hours—of course there must be many other persons employed in such a factory, in preparing new patterns, and new battery plates and connections; but these come under other heads of expenditure which have their counterparts in the present mode.

“I will now suggest a few of the plans which may be used for producing hollow vessels. The most simple process which has occurred to me, and the one which I have already used with success, is to form a block of some easily fused material into the shape of the interior of the required vessel, which serves as the matrix for depositing the copper on; when this is sufficiently thick, it may be placed on the fire till the block is melted, when the fluid material may be poured from any of its openings, and leave the hollow vessel complete and in a single piece, without the aid of the soldering iron or hammer. The temporary block on which this vessel is deposited may be formed of wax, cement, fusible metal, lead, or any other convenient material which melts at a much lower temperature than the metal to be deposited. I consider this method to be peculiarly applicable to the most complicated forms of metallic musical instruments, and to crooked tubular work generally; but it has certainly the great disadvantage of requiring a new mould for every repetition of the work, or *rather a new block*, for these may in many cases be cast in a mould which would be permanent for any number of repetitions. There is, however, one means of avoiding this in some cases, which is very well illustrated by the common *boot-tree*; if this were put together, and after having its surface metalized, placed in the depositing trough for a proper time, we should certainly have it covered with copper: we may then take out the centre or key piece, which would unlock the others, and they may readily be removed, leaving a copper boot—not a very desirable article certainly, but the block may just as well have been formed to a more suitable shape. Still there are disadvantages in this mode of procedure which would limit the application to a few articles, although there are some which could not readily be accomplished in any other way. For the production of ornamental vases and other works of value, I would suggest the following method:—Let the whole, or a segment of the work, according to the nature of the design, be moulded in wax, carved in wood, cast in plaster, or produced in any more convenient way; then metalise the surface, and deposit on as large a portion of the original as will relieve in one piece; repeat this at different times over the whole work, and so manage the edges of each piece that the whole shall key together in the usual way, so that when finished you will possess a metallic mould for the required work. Should the design be complicated in form, a number of pieces will be requisite; but there are many very expensive articles which would readily relieve if the mould were in a single piece, and a very

much more extensive class which may be made in two parts. Of course the inside of the metallic segments would correspond in polish and finish to the original model, and the metal mould, when complete, would serve for an unlimited number of copies, which would not require any cleaning up or hand finishing; but the parts would require to be put together, which I propose to effect by the same power that produced them. This, I think, may be done by covering the work with wax varnish while warm, and afterwards carefully cleaning all the edges, and then binding it together as if for soldering. The joints must now be touched over with nitric acid, and the article properly connected with the battery, and placed in the depositing trough; copper will immediately begin to deposit on the joints, and in a few hours, I imagine that these joints would be quite as strong as any other part; if so, the method would have the decided advantage of joining the work with pure copper, and of doing this without exposing any part of it to the fire. I have not yet attempted to join any work in this way; but I consider that I have good evidence of its practicability, in the well known fact of deposited copper uniting with any other portion of copper which has been cleaned with nitric acid, quite as intimately as the different parts of the original piece are united.

"In the foregoing remarks I have endeavoured to shew that the system of electro-metallic deposits is capable of producing every article which leaves the workshop of the copper-smith, and that there is no very apparent reason for supposing that it would be too costly to compete with the usual process of manufacture; still much of this opinion is speculative, and as yet I have not sufficient experimental evidence to offer in support of it; but such as I have has fully satisfied me that it is correct. The small toy which accompanied this paper was made a few days before the meeting, and I believe it is the first symmetrical copper vessel ever produced by galvanic action.

16.—*Hosking's Reversing Clutch Box.*

The remaining articles of this valuable volume are constituted of accounts of the finances of the Society.

Notice on some Experiments made with a powerful Battery of Groves' Construction. BY M. le Prof. A. DE LA RIVE.*

I have availed myself of a circumstance which enabled me to mount a Groves' battery of forty pairs, to make with this piece of apparatus a certain number of experiments, some of which appear to me to be new. I do not speak, at this moment, of any others but those which have relation to calorific and luminous effects alone; I defer speaking of those which have chemical effects for their object until they are more complete.

* Archives De L'Electricité, No. 1.

I became convinced that we cannot obtain the luminous arc between the two points of charcoal, until after the two points have been in contact, and become reciprocally heated around the point of contact. We may then, by separating them gradually, succeed in obtaining between them a luminous arc of three centimetres.. With a more powerful pile this arc is still longer. Wood charcoal, after having been brought to a strong red heat, and then soaked in water, is that which gives the most brilliant light. Wood charcoal, unsoaked in water, is not a sufficiently good conductor of electricity to allow of its employment in this experiment. As to *coke*, it succeeds as well as the soaked charcoal, but the light which it gives is not so brilliant, and more especially not so white as that which we obtain from wood charcoal dipped in water; it is always of a blueish tinge, and but slightly inclined to red.

The transportation of particles of charcoal from the positive to the negative pole, during the time that the luminous arc is produced, is evident, but is still more sensible if the experiment is made in *vacuo*. We may observe the formation of a cavity, on the point of the positive piece of charcoal, which presents the appearance of a hollow cone, into which we may almost exactly insert the solid cone which is formed by the particles of charcoal accumulated at the negative point. The phenomenon is almost the same in the air, except that the accumulation of charcoal is less strong on the negative point, in consequence of a portion of the molecules being burnt during their transit; and the positive point presents only a plain surface in the place of a hollow one. This latter effect probably proceeds from the combustion of the thin envelope of the hollow cone, which ought to be formed in the air as well as in *vacuo*.

It appears very probable that the luminous arc itself is only the result of the incandescence of the particles of charcoal transported from one pole to the other, which incandescence is accompanied by a partial combustion, and which takes place without combustion when the experiment is made in *vacuo*. The movement of these particles in the direction indicated is almost visible to the naked eye, when the current is not very strong. This transmission of the particles does not take place, as we have seen, except when the charcoal has previously been brought to a high degree of heat, a circumstance which probably, by diminishing the cohesion, facilitates the disjunction of the particles.

Charcoal is not the only body which presents the phenomenon of a luminous arc between the two points, brought into connexion with the poles. Spongy platinum, as well as copper reduced by hydrogen to an impalpable powder and then filled in tubes of glass, produce the same effect. It appears necessary that the substance employed should possess but little cohesion, in order that the molecules may be easily detached by the current.

An indispensable condition for the success of the experiment is, that the substance possessing only slight cohesion be at the positive pole, it being of little import whether the substance at the negative pole be a similar one, or a body which is highly cohesive, as for instance, a morsel of forged metal. Thus we may have a luminous arc of one or two centimetres by putting at the positive pole a piece of spongy platinum, and at the negative pole a ball of the same metal forged. We can only obtain a very short spark by adopting the inverse disposition of the bodies. We see, in the first case, that the pieces of spongy platinum become hollowed by the effect of the departure of the metallic particles, which quit it in order to be transported to the ball which is on the negative pole. It is always necessary in these experiments, as in those where charcoal is employed, that the points communicating with the poles be brought into contact, before they have the power, in being separated, to produce the luminous arch.

I have repeated the beautiful experiment of the influence of the magnet on the luminous arc which occurs between the points of charcoal. This experiment was first made by Sir Humphry Davy ; but the first idea of it belongs to M. Arago, who had been conducted to it by the analogy between the light produced by it with that of the aurora borealis, and by the observation which he had long before made, that the aurora borealis acts on the magnetic needle. We give the note which M. Arago had published before the lecture of Sir H. Davy to the Royal Society, in which the illustrious English chemist reported the experiment of the action of the magnet on the luminous arc of the pile. (*See the end.*)

I have found that, in effect, the luminous arc which is produced not only between the two points of charcoal, but also between two points of spongy platinum, or between two agglomerations of copper in powder, was influenced by a magnet, sometimes attracted, sometimes repulsed, following the pole of the magnet. The attraction or the repulsion exercised on the molecules themselves, though very distant, may be able to conduct the current ; and that which goes to prove it is, that if the attraction or the repulsion remove them too far from each other, then the current no longer passes, and the luminous effect ceases. We may thus easily cause the disappearance of the luminous arc, by merely moving the magnet.

I will not terminate this short notice without adding that I made an observation which M. Arago has, I believe, already made, to ascertain whether the light of the luminous arc which escapes between the charcoal points is not polarized. This fact is not in opposition with the idea that this light is due to the incandescence of the particles which are transported. In fact, these particles, being very distant, are isolated, so to speak, independent of each other, like the particles of gas. Now, in similar circumstances light is not polarized. It will be so if it emanates from the particles of a solid body during incandescence, such as a metallic wire,

for example, which will become heated to redness by the action of the pile.

Another remark which I have made on the light which escapes between the charcoal points, is that it may be employed as we employ the solar light, to illuminate any object we wish to daguerréotype. We obtained, by the daguerréotype, an imprint, it is true, though a slight one, but sufficiently decisive not to leave any doubt, from a plaster bust which we exposed for ten minutes to the light in question. This experiment is not easily made, because the luminous arc is intermittent, likewise that it is not always directed in the same manner, giving place consequently to shadows whose position is variable. But still, I have seen sufficient to prove that after having been reflected on a white body, the light which escapes between the two points of charcoal fixed to the two poles of the pile has still the property to act on a daguerréotype plate.

I cannot conclude this short notice without observing that the greater part of the experiments contained in it were made with the co-operation of M. M. Colloden, Melly, Suskind, and Bonigol, to whom I am happy here to express my gratitude for their able assistance.

NOTE.

"The experiments of M. Ærsted appear to me to be worthy of repetition in a circumstance which will still add to the interest which they ought to inspire, in bringing us a step nearer towards the explication of the phenomenon, hitherto so incomprehensible—the aurora borealis."

There is at the Institution of the Royal Society of London, a voltaic battery composed of 2000 double plates of four inches square. By making use of this powerful piece of apparatus, Sir Humphry Davy has observed that there was an electrical discharge produced between the two points of charcoal adapted to the extremities of the positive and negative conductors, when even these points were distant from each other $\frac{1}{36}$ or $\frac{1}{45}$ of an inch. The first effect of the discharge is to redden the charcoal. Now, immediately on the incandescence being established, the points may be gradually withdrawn from each other four inches, without breaking the intermediate light. This light is extremely bright, and larger in the middle than at its extremities; it forms, in fact, an arch.

"The experiment succeeded much better in rarified air. Under a pressure of a quarter of an inch, the discharge from one charcoal point to the other commenced at the distance of half an inch; afterwards, the charcoal points being gradually withdrawn from each other, Sir H. Davy obtained a continuous purple flame, and which measured seven inches in length.

"It is very natural to suppose that such an electric current will act on the magnetic needle just as if it was moved along a conjunctive metallic wire; nevertheless the experiment seems to me to merit being recommended to philosophers who have voltaic

great power at their command, especially with regard to the views it gives rise to relative to the aurora borealis. Will it not also be, independently of any immediate application, a phenomenon worthy of remark, that the production in vacuo, or in air highly rarified, of a flame which, acting on the magnetic needle, may be in its turn attracted or repulsed by the poles of the magnet."

ATMOSPHERIC ELECTRICITY.

Read by Mr. SPENCER, at the *Polytechnic Institution, Liverpool.*

SECTION III.

At the close of the preceding section it was stated that rain is accompanied with electricity, not only during the shower, but in much greater quantity *immediately before* it occurs. When the electricity is attracted by the earth's surface, it will arrive there much sooner than the globules of water that compose the rain; consequently, we have several indications of rain being about to fall, independent of those furnished us by the barometer.

These are often exhibited by various affections of the human frame; and little doubt can exist that they are pretty general throughout the animal and vegetable kingdoms, had we the same means of observing them. For the present I shall leave this portion of the subject untouched, as my personal experience does not furnish me with an example; but we frequently hear complaints made of the aching of old wounds, corns, increased pain from rheumatic affections, &c., all in connexion with rain being *about* to fall.

There is, however, one indication of rain that has recently come under my observation, of which I can speak more positively. Having had, within the present winter months, a good deal of experience in the new art of photography, I have been surprised in *dull weather*, even when the sky was *beginning* to lour, to find that a picture was obtained in one-third or one-fourth of the time it would have otherwise taken, and that, when such took place, rain followed *soon after*; but, on the other hand, after the rain had continued for some time, this rapidity of chemical action departed, and a picture would then take rather longer than it would on a day having the same amount of light, when no rain had fallen. If, as already stated, we associate electricity and chemical action together, or as *being identical*, the solution of this enigma is easy. To render this, however, still clearer, I shall enter somewhat more into detail, and, while doing so, will explain, perhaps, more fully the immediate cause of rain as it occurs, under ordinary circumstances, in our climate.

When the condition of the atmosphere admits masses of cloud to float near the surface of the land or ocean, the electricity contained in them becomes attracted by either of these bodies, and, in consequence, continued streams of electricity are given out of the cloud

towards the earth. This electricity, under ordinary circumstances, is unseen, the distance between the attracting body and the attracted being so considerable, and the attraction being equal over such an immense area as that in which rain occurs, renders the electricity given out of the most feeble possible tension, but not so much so that it cannot be detected by our most delicate instruments: indeed, sensitive electrometers betray evident marks of electrical disturbance prior to the fall of rain. When a sufficient quantity of electricity is thus discharged from the cloud, that the small globules composing it cannot hold each other in electrical repulsion, they condense into larger drops, and, from their specific gravity, fall as rain, but still surrounded by a feeble atmosphere of the subtle fluid, which descends with them to the earth. This latter portion of electricity, so retained, may be familiarly likened to the residuum that is left in the Leyden phial after its first discharge, a feebler quantity being always detected. It sometimes happens that masses of cloud, not in the highest possible state of electric tension, are discharged without noise, by a shower of rain falling through them from a mass of cloud in an upper stratum of atmosphere. In such cases, the weather is what is termed *thick* and *close*, and the small dropped rain is attended with flashes of harmless sheet lightning unseen during the day.

In tropical climates, where evaporation takes place in much greater degree than our own, immense masses of thunder cloud are sometimes discharged by the attraction of a mass of negative cloud in a higher or lower stratum of the atmosphere, often floating immediately above or below it, as the case may be. In such instances the lightning is discharged in almost vertical streams, and spouts of water, caused by the sudden condensation of the globules forming the cloud, are discharged with fearful rapidity to the earth: these having been known to cut circular basins, in a gravelly soil, to the depth of six or ten feet, with as much precision as by an artificial process. They have also been observed to take a progressive motion, and, in those instances, a deep trench has been cut for many hundred yards; yet, so sharp have been the edges of such cuttings, that loose leaves have been left undisturbed on their margin. These land spouts are always observed accompanied with lightning, streaming more or less vertically, and, reasoning from the foregoing, it must be as necessary a result as the developement of the latent caloric itself.

The phenomena of water-spouts next demand our attention.

From all the accounts of observers I have been able to collect, there seems no doubt, in my own mind, that water-spouts are merely an extraordinary effect arising as corollary out of the principles already laid down, and may be, on the same ground, very satisfactorily explained. Most observers state they are attended with sharp claps of thunder. There are many other coexistent circumstances that point out their electrical origin.

Suppose we take by no means an unusual meteorological state of things, and reason from it. Let us suppose an immense storm cloud filled to repletion with free electricity, as we have already seen all clouds are, in greater or less degree. Such a cloud will be positive as respects the surface of the ocean, evaporation being always going on to render it so, coupled with the known fact, that aqueous surfaces are the best possible conductors. In consequence of this state of things, an immense attractive power will exist between the cloud and the surface of the ocean, until the cloud is gradually drawn from its altitude and begins to approach the surface of the water. This approach would seem, by the current testimony of close observers, to be accelerated as the distance decreases, and there can be little doubt that it obeys the well known law, the air being still, of an increased velocity of approach as the squares of the distance decrease. Now, by paying attention to the preceding statement with respect to clouds in general, it is sufficiently obvious what, under the circumstances, *must* take place. Although the under side of the clouds towards the earth or sea are comparatively level, yet there are irregularities enough to render some portions of them nearer the sea than others. A few yards would disturb the attracting equilibrium sufficient to cause an ultimate discharge at the point where the irregularity took place. Let us suppose that a rag of cloud hung down, and, by the way, this is in strict accordance with the observed facts, as also are all the other phenomenon I shall predicate. On the other hand, any wave that lifted up its head on the ocean beneath the point where the shred of cloud impended, would accelerate or induce the spout at that place.

Whenever these two points would come within discharging distance, the electricity contained in the cloud would rush towards the sea: the water contained in it would necessarily discharge itself at the same point. We have here the whole phenomenon of the water-spout. Sometimes three or four have been observed at one time, and all proceeding from one cloud: at other times, a boiling up of the ocean has been observed immediately under the spot where the spout was about to take place, as if there was a desire in the ocean to meet the spout at that particular point. This is exactly what should take place, and, had the particulars of a water-spout never reached me farther than the bare fact, I could, with the greatest ease, have predicated that such would take place—that it would be a necessary result of what has been already laid down. It is, however, this fact alone that has rendered the phenomena of water-spouts apparently inexplicable, as it induced some observers to pronounce that the rush of water was from the sea upwards, and not from the cloud and downwards. The cause, however, will, I trust, be obvious. Bodies dissimilarly electrified attract each other. The cloud being highly positive on the one hand, and the ocean being in an opposite state, a mutual attraction takes place between them, and were it not for the high specific gravity of the water, as

compared with the opaque vapour of the cloud, it would ascend still higher. Similar attractions are often observed on the land even on a much smaller scale. While making these observations on water-spouts, I wish it to be understood I do it with great deference, because I differ with a high meteorological authority, Mr. Espy, on this point; and that water occasionally rushes *upwards* from the sea it would be difficult to deny, or how else are we to account for showers of fishes and other marine productions that are sometimes precipitated on the land? This phenomenon is accounted for, by this gentleman, by supposing a vacuum has been created immediately above the spot where the spout takes place, in consequence of the discharge of latent caloric during the process of cloud forming.

In the present state of my knowledge, I cannot, by possibility, conceive that this is adequate to produce the observed effect, more especially when we find water-spouts taking place when all is comparatively calm for many leagues around. Were there a rush of air, *in* and *upwards*, observed within any reasonable distance from where the phenomenon was occurring, then we might assimilate it with the tornado; but, on the contrary, water-spouts have been observed at the mouths of some of the narrowest African rivers, while the vegetation at each side remained comparatively quiescent; even vessels have entered between the spout and the land without any observable difference in the breeze which bore them onward.

There are two other great meteorological phenomena which I must briefly explain: to enter into each of them fully would require much more space than is at present allotted to me. The aurora borealis and australis are explainable, in my opinion, perfectly on the principles I have laid down. Evaporation takes place at the equator in greater degree than at any other portion of the earth; and, from the intensity of the sun's rays at the same point, the atmosphere is, by expansion, eight miles higher than at the poles.

When clouds are formed within the tropics, they are carried to the higher regions of the atmosphere, where they are rolled off, down this inclined atmospheric plain, towards the poles. Thus far, this is no theory, but is now demonstrated by meteorologists. An immense mass of cloud is accumulated in the regions toward the poles, where evaporation does not go on to an extent sufficient to cause a hundredth part of that found there. This cloud is much nearer the surface of the earth there than at any other point of the globe, from the causes just named. It is highly surcharged with electricity, a greater quantity of cloud being there condensed in a given space. Although those clouds are so highly charged with electricity, yet we seldom have thunder-storms in the arctic regions; and, from what has been said, this may appear paradoxical; yet, a little reflection will shew its strict accordance with the laws laid down. The clouds are all positive, or nearly so, with respect to each other, and hence there is nothing to disturb their equilibrium,

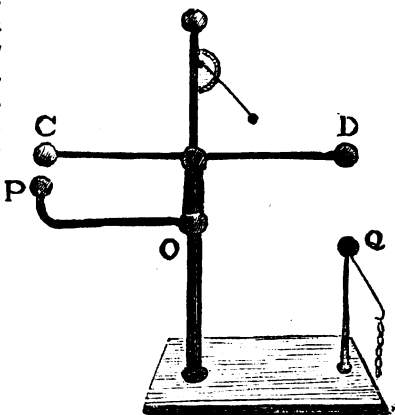
it being only where we have negative clouds relatively that we can have thunder-storms. Still, these positive clouds discharge their electricity. This discharge is continually taking place, and in much greater degree when unseen than when observable by the human eye. During fog, and rain, and snow, it descends on the particles of those bodies; but, when the air is dry, and, consequently clear, the attraction of the earth causes the electricity to be discharged from the clouds; but, the air being a bad conductor of this agent, it is rendered visible to the eye in its descent to the earth, and it is only when it is drawn from the upper regions that it is visible: when it comes near the earth, it becomes so diffused that the eye loses it. We have thus the phenomenon of aurora borealis and australis, as the same causes exist at the other pole. There are a multitude of observed facts that all tend to prove this, and it would be somewhat difficult to ascribe it to any other cause, if we take into consideration that clouds are bodies, under all circumstances, charged with electricity.

ELEMENTARY LECTURES ON ELECTRICITY, &c.

LECTURE XV.

In pointing out the particulars to be attended to in the employment of the electrical battery, I shall, in the first place, observe, that if the battery be charged to a high degree of intensity, and discharged in the usual way of discharging single jars, and especially if the circuit between the inner and outer surfaces be short, one, or more, of the jars is almost sure to be broken, and the battery, consequently, so far spoiled; and it cannot be charged again until the broken jar, or jars, be removed. Thence the necessity of watching, carefully, the progress of the charge, so that we may make the discharge before the intensity gets too high. To prevent accident, by inattention or otherwise from too high a charge, Mr. Cuthbertson invented a *discharging electrometer*, by means of which the battery will discharge itself at any required intensity, without depending on the attention of the operator.

Figure 3 is a representation of Cuthbertson's *discharging electrometer*. It consists of a stout mahogany board as a base, from which rise two glass pillars, one of which is rather stout and the other only slender; the latter is surmounted with a brass ball, into which is screwed one end of a stout brass wire, to which the outside of the battery, either directly or indirectly, is to be connected. The other glass pillar carries vertically a wide brass tube, terminated at



each end by a hollow brass ball, into the lower part of which is inserted, and firmly cemented, the upper end of the supporting brass pillar; and into one side of that brass ball is screwed a horizontal brass tube or stout wire, bent upwards near the outer end, and terminated by a brass ball, as seen in the figure. Through the middle of the upper large brass ball passes a horizontal brass rod, terminating by a ball at each extremity. This rod is nicely balanced on a knife-edged fulcrum in the interior of the large hollow brass ball; and the arm which projects over the lower bent arm is graduated, and finished with a sliding weight, which, when placed at different distances from the fulcrum, loads the arm with a proportionate weight, from one grain to sixty grains. The ball of the graduated arm of the balance rests upon the ball of the lower bent arm. The ball at the other end of the balance will then be four inches from the other ball which surmounts the slender glass pillar. If, now, the metallic part of the instrument, supported by the stout brass pillar, be connected with the prime conductor, or the interior of the battery, then, while the latter is discharging there will be a repulsion between the ball of the bent arm and that of the balance which rests on it; and, at the same time, an attraction will take place between the other two balls; and when these repulsive and attractive forces overcome the weight at the other end of the balance, the attracted arm will descend, and discharge the battery on the lower insulated ball. Now, as the attractive and repulsive forces will always be proportional to the intensity of the charge, and as these forces have to overcome the load at the other end of the balance, that load, whatever it may be, is a proper measure of the intensity of the charge. The instrument is usually surmounted by a quadrant and electrometer, as shewn in the figure, as an indicator of the progress of the charge, which is not shewn by the discharging electrometer itself.

It is obvious, from a consideration of the beautiful arrangement of the various parts of this instrument, of the principles upon which it operates, and of its functions as a self-discharger at any required intensity, that it is well calculated to relieve the anxiety, and dispense with much of that attention of the experimenter, which he invariably labours under by the employment of the ordinary discharging-rod alone; for the sliding weight being once adjusted on the horizontal arm, for any given intensity, and the body to be operated on properly placed in the circuit, the experimenter's care is at an end, as the machine has (now) only to be worked till the discharge takes place.

The mechanical action from a discharge of an extensive battery of jars is exceedingly great, and bodies of every kind, whether conductors or nonconductors, can be perforated, torn, or even shattered to pieces by means of it. If, for instance, we now place a quire of paper vertically between the two balls of the universal discharger, and arrange this apparatus so as to bring the paper into the circuit of the battery when charged to about 80° or more, per quadrant

electrometer ;* the electric fluid rushes with violence through the quire, and tears the outer leaves to a considerable extent, whilst the inner ones are sometimes partly torn, but more frequently perforated in several places. Several thick cards may also be torn in a strange manner by a discharge from the battery ; and if the paper or cards be made slightly damp, the ruptures made by a discharge are frequently much greater than when operated on in a dry state ; and as the moisture communicated to them enhances their electric conduction, a greater number of plies can be perforated when so treated.

The electro-mechanical and electro-calorific effects are sometimes so intimately connected with each other, that it would be impossible to separate them by any mode of experimenting hitherto known. For, although there be some singular exceptions to the general rule, it generally happens that the greater the mechanical action the more favourable is a discharge in accomplishing the ignition of bodies through which it is transmitted.

Let us, for instance, employ the force of a single jar, when charged to a high intensity, upon a narrow strip of gold leaf placed between two dry cards. The whole of the circuit, I should wish you to observe, is of good conducting materials—metals. Now, the discharge being made, we examine the result on the strip of the gold leaf, and find that the metal has entirely disappeared ; but both cards are stained with a beautiful purple or violet-coloured tint, very curiously distributed, and extending over nearly the whole of the two inner surfaces of the two cards, being most dense, and of the deepest colour along the middle, where the gold leaf was placed, and becoming more and more attenuated thence to the outermost skirts of the stain.

Now, by paying attention to the complicated results of this experiment, we first discover that the metal is destroyed, or, in fact, deflagrated, and thus, by oxydation, has assumed a different colour : and we next observe that the mechanical action has distributed this oxyde over a much larger space than the gold originally occupied. And what is particularly remarkable, these mechanical effects are chiefly produced *laterally*, and but very little, if any, are to be observed longitudinally, or in the direction of the discharge. I shall, however, as we proceed, endeavour to vary this experiment, and shew the effects by different arrangements of the apparatus. At present, I offer to your notice but one or two variations, which will be sufficient to convince you that when the electro-mechanical action is sufficiently abated, the gold leaf suffers no change, even from the most formidable electrical charge that we are capable of producing.

The experiment by which I am about to prove this fact, will differ

* As there are scarcely two of these electrometers which give the same indications for any given intensity, 80° will not express the requisite intensity with different electrometers ; I should, therefore, wish it to be understood, that as the mechanical effects depend more on the *intensity* than on the *quantity* of fluid employed, the intensity for purposes of this kind should always be considerable.

in no other respect from the last one, than by the introduction of an inferior conductor to the circuit whilst discharging the jar. The inferior conductor which I employ for this purpose is simply a piece of wet thread, about eight or ten inches long : it forms a part of the circuit, and will be the means of lessening the mechanical action, and you will observe that, by this arrangement, the gold leaf remains uninjured by the discharge of the jar through it, although the intensity of the charge was as great as in the former experiment.

As it is still possible that you may require further evidence respecting an abatement of the mechanical action by such an arrangement as that last employed, I will again vary the experiment, to convince you of this fact, and to shew you that the calorific effects on the gold have a dependence on the electro-mechanical action in the manner I have already stated.

In this variation of the experiment I will make a slight opening in the metallic part of the circuit, and in that opening I will place some light seeds, which, as you already know, would be blown away were I to discharge the jar through them when the rest of the circuit is metallic : but now, as the discharge has to pass through the inferior conducting wet thread, you will find that the seeds remain almost unmolested ; they are not scattered abroad, as before, and the only disturbance which they experience is a mere consequence of some trifling attraction which takes place between them and the ends of the wires where the metal circuit is interrupted. The gold leaf, you will observe, is not in the least changed.

We might now vary the experiment in another manner, by dispensing with the wet string, and making the whole circuit metallic, with the exception of a small opening where small seeds may be placed, as in the last experiment. Now, in this case, we have a fair opportunity of comparing the mechanical action on the seeds, with that of the last trial ; and you will find that the seeds are blown away, and the gold leaf is destroyed.

I know of no experiments by means of which I could shew you that the calorific effects on the gold depend on the electro-mechanical action of the discharge.

However, I have still one other variation of the experiment, which I shall now proceed with. You are aware, that when a jar is discharged, in the usual way, through any person, a violent shock is produced ; but if I place the wet thread in the circuit, no shock whatever is experienced. Now, by placing myself, the wet thread, and the gold leaf in the circuit, I discharge the jar from a high intensity, without producing any effect either on the gold leaf or on myself.

Description of a Voltaic Battery, in a Letter to the Editor from
Mr. E. WENCHEBACT.

Amsterdam, 16th. Feb., 1842.

Sir,—The Annals of Electricity, Magnetism, and Chemistry for December, 1841, contains the offer of a prize volume for a voltaic

battery that "will decompose acidulated water at a greater rate, and with less cost, than by any hitherto made public. The time of action not to be less than four hours."

I now take the liberty of addressing you a few lines, for the purpose of stating that the effects of a battery invented by a friend of mine, are able to answer to the above-mentioned question. Not exactly knowing what quantity of decomposed water already has been obtained in your country, and therefore not being able to make any comparisons between them, I will at once submit his results to your more perfect knowledge, and shall be highly obliged if the trouble I am now making to you, can be rewarded by the results themselves.

To W. Sturgeon, Esq., Royal Victoria Gallery, Manchester.

The battery in question consists of 10 jars, each of which is constructed by an ordinary vase or vessel, wherein a cylinder of amalgamated zinc ($\frac{1}{8}$ of an inch thick) is placed, and fit for receiving a porous diaphragm of earthenware with a piece of coke in it. The coke and zinc are connected together by a thick copper wire, soldered to the zinc, and in full contact to the coke by means of a hole filled up with mercury. The coke being of a very irregular form, it is somewhat difficult to ascertain its surface accurately, but from estimate I think it certainly does not exceed 300 square inches (English) by the whole battery. The room between the coke and earthenware is filled by acidum nitricum, whilst the zinc cylinders are placed in water with a few drops of sulphuric acid in it. The same effect will be produced when the room is filled by sand, and the liquids thrown upon this till the sand becomes quite wet. The last method, certainly, is the more economical.

The decomposition of water goes on at a rate of 384 cubic inches in four hours, without the least interruption, the action of this battery being so constant, that Prof. Van du Boon Mesch, at Leyden, to whom one of the jars has been sent, ascertained that its action, though with less vigour, continued for eleven days.

The consumption of zinc is very small, and the evaporation of nitric acid only takes place when the poles are connected. All the other effects of heating, burning charcoal, lighting, &c., are very strong, and equally gratifying when first tried. The only remark which is to be made is, that notwithstanding every piece of coke will have the same effect, the sort which is known by the name of carbon is the most fit for the purpose, because it is more tight, and not so very hard that a carpenter should not be able to bring it into a regular form by means of a saw.

Certainly many persons will apply for competition, and perhaps some of them may still have been more successful than my friend; should that be the case, cast this letter aside, and only when you think it fit for competition or publication in the *Annals*, you will highly oblige my friend, whose name I am ready to make known to you when admitted for competition, and as soon as you will do me the favour of a few lines in answer to my undermentioned address.

In hope I have not too long detained you, and for excuse of the liberty I have taken,

I remain, Sir, your very obedient servant,

E. WENCHEBACT.

P.S.—Even should the same result be obtained by a gentleman in your country, it hardly will be by a battery more economical than this employed by my friend, because the whole cost of it does not amount to £1 8s.

Improvement in the Daguerreotype Process of Photography. By F. A. P. BARNARD, Prof. of Math. and Nat. Phil. in the Univ. of Alabama.

(From the American Journal of Science and Arts.)

Messrs. Editors,—I commenced, about a year since, in connection with Dr. Wm. H. Harrington, of this place, a series of experiments in photography, according to the methods of Mr. Fox Talbot and M. Daguerre. Our attention was directed principally to the Daguerreotype process. From the analogies known to exist between iodine and chlorine, we were strongly impressed with the belief that the latter substance might in some manner be employed to render the surface of silver more sensitive to the action of light than it had yet been made. To determine the correctness of this opinion we instituted a variety of experiments, which, as they proved for the most part unsuccessful, it is unnecessary to detail. The coating formed by the direct action of chlorine gas upon polished silver, was not found to possess the desired photogenic properties. We were led, therefore, to seek whether by the decomposition of some compound of the metal, a sensitive chloride could not be produced. Mr. Talbot had already done this in the preparation of his photogenic paper; but a sit was our desire to avail ourselves of the beautiful lights formed by the vapour of mercury, and as the prepared paper, at least so far as our experimenis go, is not susceptible of receiving them, we endeavoured to produce the decomposition upon the surface of the solid metal. It occurred to us as a possibility that the iodide formed in the usual manner, by exposing a plate over the vapour of iodine, might perhaps give up its silver to chlorine, and thus produce the desired coating. This impression was not verified in our first experiments, owing to a cause which will presently be noticed. Perseverance, however, at length brought its reward. By varying, in every possible manner, the circumstances of the experiment, we succeeded in producing a surface so exquisitely sensitive to the action of light, that the image of an illuminated object was formed upon it in the camera in a space of time almost inappreciable.

The following is the process by which this result is obtained. Let the plate be prepared in every respect as if an impression were to be taken according to the method of M. Daguerre. Let it be then exposed for the space of half a minute to the action of chlorine gas, diluted with common air to such a degree that it may be inhaled

without any particularly unpleasant sensation. It will then be found so extremely sensitive, that, on being placed in a camera, with an aperture such as is commonly employed in taking miniature portraits, an impression will be produced upon it in the smallest time in which it is possible to remove and replace the screen. The completion of the picture over mercury is effected in the usual way.

A plate thus chlorized, on exposure to light, almost immediately assumes a very deep violet colour, nearly approaching black. The mercury is not directly tarnished, and in this state the picture is even more beautiful than after being washed with the hyposulphite of soda. But without this washing it cannot be preserved.

M. Daguerre has announced that he is able to take images of objects in an instant of time. I have not seen any statement of his method. Some of the artists in the Atlantic cities have been equally successful. Their process is not that which I have here described. I suppose that I am acquainted with the mode of preparation which they employ; but as it was communicated to me under an injunction of secrecy, before I had discovered it myself, although I had actually employed it unskilfully, and therefore without complete success before, I can say nothing of it here. It will, without doubt, soon be made public, if it is not already known. I believe, at any rate, that the chloride coating is more sensitive than any other which has yet been used.

This discovery has opened a new field of experiment, in which we are now actively engaged. The results may be communicated hereafter.

It appears to us that the lights produced by this process of preparation are much finer and smoother than those of the original process of M. Daguerre. Some idea of the quickness of the camera operation may be formed from the statement of the fact, that a man walking may be represented with his foot lifted as about to take a step.

The quantity of chlorine necessary to produce the effect is exceedingly minute. In our early experiments we employed a quart bottle of the gas, opening it in a deep box, and leaving out the stopper while deliberately counting twenty. Replacing then the stopper, the plate was laid for half a minute over an opening in the top. After fifty experiments the gas in the bottle seemed not to have lost any of its original intensity of colour. We have better arrangements at present in preparation.

Much care is necessary to avoid an excess of chlorine. The principal cause of our early failures arose from an error of this kind. One may easily determine, with any apparatus, the time and quantity necessary, by laying a plate over the aperture and drawing it partially off at intervals. The action of the gas will then be greatest, of course, upon the part longest exposed. Too much care cannot be taken to exclude the light during the process of preparation.

Tuscaloosa, July 1, 1841.

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ROYAL VICTORIA GALLERY, MANCHESTER,

On the Chemical Relations now subsisting between Plants and Animals, with reference to those which have subsisted in former ages. In a Lecture delivered at the Conversazione, on Wednesday, March 9th, 1842. By Dr. LYON PLAYFAIR.

It is not an idle task to cast a retrospective glance to ages far beyond human ken, nor to try to discover the physical and chemical causes which then regulated the production and maintenance of animal and vegetable life. Races of animated beings once lived and performed their destined functions on the earth, but they have now passed away for ever. Ages rolled on, and other races were called into being, which have again disappeared, and yielded their places to new forms of organic life. Geologists have examined their remains, and endeavoured to form conceptions of the physical characters of the globe during their residence on it. They have drawn legitimate conclusions, from the order of their occurrence, regarding several grand eras in the history of the world. These inferences have been deduced from external characters possessed by animals and plants occurring in particular formations; and, by a happy application of analogical reasoning, they have shewn what must have been the aspect of the face of nature when these plants and animals existed. Why, therefore, should the chemist be afraid to follow in their footsteps, and by evidences drawn from the chemical composition of matter, lend his aid in explaining the mighty revolutions which have taken place upon the surface of the earth? True, his science is not speculative, nor does he love to waste his time in the vagaries of theoretical speculation; but, we believe, that the evidences existing on the earth are sufficient to form a basis for inductive reasoning, without the necessity of substituting ideas for facts.

I shall, therefore, endeavour to lay before you this evening, an *Ann. of Elec. Vol. VIII. No. 46. April, 1842.* *

account of some of the chemical causes which have led to the developement of the various races of animals and vegetables, which have appeared and disappeared in their course. And be it remarked at the threshold of our enquiry, that by the term *chemical* causes, we do not at all mean to undervalue the *physical* causes which have lent their aid or been paramount, in their developement or extinction, but merely employ the term to form a boundary in the examination of a boundless subject.

But, before I can make myself understood, it is absolutely necessary that you should be acquainted with the laws which regulate the nutrition of the vegetables now existing on the earth. Mr. Ransome, in a series of lectures, has so ably performed this task, that little remains for me, except to refresh your memory with a few of the grand laws connected with vegetable nutrition. You are all aware that organic matter, in general, is composed of four substances, carbon or charcoal, oxygen, hydrogen, and nitrogen. You know, that when charcoal unites with oxygen, it forms a gas familiar to you as the air which escapes in small bubbles from champagne or beer, and which has received the name of carbonic acid gas. You know also that when oxygen unites with hydrogen, the familiar substance, water, is produced; and that when the latter element (hydrogen) enters into union with nitrogen, ammonia or hartshorn is the product.

Now, these three compounds—carbonic acid, water, and ammonia—form the food of plants. They all exist in the air, from whence they are extracted by the vegetation which covers the earth. The grand difference between animal and vegetable life is, that this carbonic acid, which is fatal to animals, is the primary food of plants; and that whilst plants are continually inspiring this noxious gas, animals are as constantly expiring it. There are certain constituents in the food of man and animals, which are not at all calculated to assist in the nourishment of the body, but which are, nevertheless, indispensable in supporting the process of respiration. Such are starch, sugar, and gum. I had the honour, on a former occasion, of explaining this to you in detail. These substances, in supporting respiration, are converted into carbonic acid and water. Hence it is that animals continually expire this gas. But there are other substances also exclusively adapted for the nutrition of the system, and which, when present in superabundance, are separated as excrementitious matter. In such substances nitrogen abounds. Hence during their decay, ammonia is generated. When animals die, their bodies enter into a state of putrefaction, or, to speak more correctly, the constituents of which their bodies are composed, change their form, and are converted into carbonic acid and ammonia.

Conceive the thousand millions of men who inhabit the globe, and the myriads of animals which teem on its surface—all sending into the air, every day, vast quantities of that noxious gas. Conceive the vast amount of fuel constantly generating the same com-

pound, and it is obvious that the earth would soon become uninhabitable were there no means of removing it from the atmosphere. Nor is this all. As carbonic acid is fatal to animal life, in as great a degree is the oxygen of the air necessary for its support. But this oxygen is always withdrawn from the air as carbonic acid is formed; for carbonic acid consists of carbon and oxygen. Thus, in burning, ten cwt. of coal consumes 32,000 cubic feet of oxygen gas; so that this town of Manchester (calculating its inhabitants at 300,000, the round number of the former census) consumes, for domestic purposes alone, exclusive of the manufactories, no less than 23,614,285,714* cubic feet of oxygen, and sends into the air a like quantity of pestiferous carbonic acid in its stead. Again, each man consumes and corrupts, every day, twenty cubic feet of air; hence the population of Manchester, by the air consumed in breathing, corrupt 2,190,000,000† cubic feet of air every year.

But vast as these quantities may appear, let us consider what a fraction of the globe we form; and not to extend our ideas beyond what they will readily embrace; let us calculate what 100 million men (an insignificant portion of the population of the globe) will annually consume and corrupt of air.

One hundred million men, then, will render unfit for the support of animal life, every year, no less than 9,505,200,000,000 of cubic feet of air.

From such data as these, it is evident, that the air would soon be rendered unfit for the support of animal life were there no means by which it is retained in a state of purity.

This it is the duty of plants to perform. An All-wise Creator has connected the life of plants and animals most closely together, the one depending upon the other. It is, indeed, a wonderful link which associates the vegetable and animal kingdoms. Plants form the primary nutriment of all animals; for although there are certain kinds entirely carnivorous, still the herbivorous animals upon which they subsist receive their nourishment from vegetable matter. But, following the chain in its continuation, we discover that animals, on the other hand, furnish the food of plants. During their life, they constantly expire carbonic acid, and discharge from their system matter, which by its decomposition, emits ammonia into the atmosphere; whilst at their death, their bodies decay, and furnish the same substances to the surrounding air. Thus they afford food for

* The calculation is as follows:—It has been found that a small town of 7000 inhabitants consumes in fuel, for domestic purposes, 551 million cubic feet of oxygen.

$$\frac{551,000,000 \times 300,000}{7000} = 23,614,285,714$$

† The calculation is as follows:—Supposing that a man consumes 20 cubic feet of oxygen, each day, in the process of respiration, then $365 \times 20 = 7300$. Again, $7300 \times 300,000 = 2,190,000,000$.

those very plants, upon which they themselves subsist ; thus also the destruction of an existing generation forms the means for the production of a new one, and *death* becomes the source of *life*.

Such is a brief outline of the intimate connection subsisting between the animal and vegetable kingdoms. So closely, indeed, are they bound together, that, in the present system of things, we could not conceive the existence of one without that of the other.

But very different seems to have been the arrangements which prevailed in former ages. Animals were not then created in number sufficient to supply the food of plants. And although we cannot affirm, or even suppose, that the animal kingdom was independent of the vegetable, we have evidence enough to show, that the latter was in no wise dependant for support upon the former. If it be allowed that the paucity of animal remains, even in the carboniferous strata, where plants so abound, proves that the balance betwixt animal and vegetable life now subsisting, was then unknown, we must suppose that the food of plants was then supplied from other sources.

It matters not what vague speculations have been held regarding the cosmogony of the world. With these the chemist has nothing to do. His science treats only of matter and its properties. Nor can he, without ample demonstration, listen to specious notions propounded of the transformation of the elements of which it is composed, or admit these as a basis for induction. His science teaches him that there are fifty-five bodies on the earth, by the different combinations of which all the varieties of matter are produced ; and further, in the present state of his knowledge, he must allow that all these elements were on the globe when its present position was assigned to it.

However different in intensity and in form may have been the causes which produced the mighty yet gradual revolutions of former times, we discover in them a close analogy to those of the present day. Hence we are not warranted in presuming that the food of plants and animals was different then from what it is now. So far only can we affirm, that the food of plants was received from other sources than at present.

The food of plants consists of water, carbonic acid, and ammonia. What proofs can we find of the existence of these in former periods of the earth's history ? During the deposition of the primary strata, where as yet no traces of plants and animals have been found, it is evident that carbonic acid existed. The primary limestones furnish sufficient evidence of its existence. It is true that the partial and irregular distribution of these limestones prove that their deposition was due to limited and local causes ; but this does not militate against the idea that the carbonic acid must have been generally diffused throughout the atmosphere. All the limestones of aqueous origin have evidently been deposited from a solution in water containing an excess of carbonic acid ; for without this ex-

cess carbonate of lime will not dissolve. Now, without entering into geological speculations, it seems to be pretty conclusively established, that the heat of the ocean was considerable during the deposition of the earlier of the primary strata. From the contortions in the laminæ of greiss, mica schist, and chlorite schist, the water from whence they were deposited appears to have been in a very troubled state, just as it is in the present day when in a hot vessel. Now, when in this hot state it could not absorb or retain in solution carbonic acid from the air. Only in those spots where local causes had reduced the heat, carbonic acid might be dissolved, and carbonate of lime formed; or, if pre-existing, brought into solution, and afterwards deposited. This heated state of the ocean might, in some degree, account for its saline impregnation. There is no evidence of land having existed during the primary period, and during the ages which constituted it, this heated water acting upon the rocks which formed the crust of the globe, would have a wonderful effect in promoting chemical combinations, and dissolving the various soluble materials contained in these rocks. In all strata of later origin than the primary, we find rock salt. Salt springs arise from the coal system—rock salt abounds in the new red sandstone. The Alpine salt works are in the oolitic system; those of Cardona in the greensand; and of Wieliczka in the tertiary rocks. (Phillips). But, as far as I am aware, rock salt is absent from the primary strata. This seems to indicate that the water had not obtained its full saline impregnation when those strata were deposited. And indeed, it seems not improbable, that during this era, the water obtained many of its soluble salts from the disintegration of the primary rocks.

But to return from our digression. The existence of the limestones proves that even in the primary period, carbonic acid existed. The peculiar smell which distinguishes hornblende, and various aluminous minerals, when they are moistened, is due to minute traces of ammonia contained in them. Now this smell is very perceptibly possessed by the hornblende schists, which accompany gneiss; and hence we have a right to conclude, that they absorbed this compound from the air, or from the water, during their deposition. There are many facts to prove that ammonia was formerly of inorganic origin. Not only is it a constituent of all aluminous and ferruginous minerals, but it exists in many natural products found in volcanic regions. Dr. Daubeny has supposed that all the carbonic acid and ammonia which now exists, or has existed in the atmosphere, may have been derived from the interior of the earth. He finds a difficulty in conceiving how the hydrogen and nitrogen could have been made to unite on the surface of the globe, and hence, draws them from its interior. But although we do not agree as to the source from whence the food of plants is derived, we both equally admit that the primary food was of *inorganic* origin.

But I must assume, for the present, on the supposition that these

bodies were original constituents of the atmosphere, that they both existed in the air, during the primary periods, or, at all events, antecedent to the secondary, in much larger quantity than they do now : and the proofs are obvious. Consider the immense deposits of vegetable matter in the carboniferous strata. All the carbon and nitrogen of which this is composed must have been originally present in the air as carbonic acid and ammonia. The greatest part of this vegetable matter is carbon ; and hence it follows, that, as the carbon of the carbonic acid was retained, and its oxygen liberated, the air of the present day must be much poorer in carbonic acid, but much richer in oxygen, than that of former ages. Dr. Daubeny denies the possibility of ammonia ever having been present in the air in much larger quantity than at present ; and, in proof of his view, he cites the experiments of Christison and Turner, that $\frac{1}{100}$ of ammoniacal gas in air acts as a poison to the vegetable kingdom. To this it may be replied, that ammoniacal gas is never present in the atmosphere. It is always in union with carbonic acid as carbonate of ammonia ; and it is well known that water impregnated with this salt in the proportion of 30 : 1, is very beneficial to vegetation. One pound of rain water contains rather less than $\frac{1}{4}$ grain of carbonate of ammonia. Now, even supposing that the rain of former ages contained 800 times this quantity, it would not be prejudicial to plants, but administer to the luxuriance of vegetation. Again, it has been found that plants flourish with great luxuriance in an atmosphere containing as much as $\frac{1}{12}$ its bulk of carbonic acid. But the air of the present day contains only one volume of this gas in 2,000 volumes of air. Hence it follows, that the air of former ages may have contained more than 150 times this amount, without injury to vegetation. These facts are sufficient to prove that the air formerly may have been much richer in the food of plants. Brogniart believed this to be the case many years since, and founded upon this view some ingenious speculations. Doubtless an atmosphere with quantities such as we have mentioned, would prove fatal to animal life, but we will shortly show that the express duty of former plants was to prepare the world for the reception of animals, and finally, for that of an intellectual being. One question, then, only remains : from whence did the atmosphere receive this carbonic acid ? Dr. Daubeny conceives by a gradual evolution from the interior of the earth. He supposes that this carbonic acid was furnished to plants during their life—not that a certain amount was originally emitted into the great magazine of food—the atmosphere. But it is difficult to conceive that an All-wise Creator would have made the life of plants and animals dependant upon adventitious circumstances. And surely it cannot be averred that the emission of carbonic acid from volcanic sources, would be regulated by a direct interposition of a Divine Providence. The vegetation of the globe did not at once spring into existence, but was as slow in its progression, as that of animals ; so that during the ages, when a scanty vegetation

covered the earth, the carbonic acid must have been constantly accumulating. And if, at its commencement, there was sufficient carbonic acid for the wants of plants, in the course of ages it must have been accumulated in much larger quantities. So that even on this view we must admit, that the proportion in the air could not be kept in any constant quantity. But we do not see any strong reason to suppose that the original food of plants was evolved from the bowels of the earth; nor can we discover any strong objection to the hypothesis, that all the carbonic acid and ammonia from the beginning of time, were original constituents of the atmosphere. The nitrogen and oxygen of which the atmosphere consists were certainly not evolved from the interior of the earth; and yet upon the same grounds could this be affirmed, for nitrogen, like carbonic acid, is evolved from the volcanoes of the present day. It is indeed difficult to conceive from what stupendous magazines of carbon the carbonic acid of the air was formed, or how hydrogen became united with oxygen to form all the water which covers the earth, without the occurrence of such dreadful explosions as would dash the the present system of the world to fragments. Such stupendous operations of nature are as yet beyond the capacity of the human mind to fathom: just so with the ammonia. If we cannot comprehend the mighty power which deprived its elements of their elastic condition, we can only believe that that power is as yet undiscovered.

From all that has preceded we believe we are warranted in considering that the carbonic acid and ammonia in the air were original constituents of the atmosphere; and further, that in former ages, they were present in much larger proportion than at the present time. We have shewn that such an atmosphere would not be unsuitable for vegetable life, although it would, certainly, prove destructive to terrestrial animals. We have now to examine the economy of vegetable and animal life of former times.

The first certain proofs of organic life occur in the *grauwacke* series. It is certainly singular that animals occur in this series, where plants are not found. These animals are not merely zoophyta, but *conchifera*. Still we cannot suppose that sea weed did not exist to supply them with food; for the spongy texture of such plants make them prone to decomposition. The paucity of animal remains at this period, and their very unequal distribution, indicate that the conditions necessary for the support of animal life were not yet generally established. As we reach the *silurian* rocks we discover that the conditions for organic life have become more favourable, and consequently we find a much greater variety of forms; but we are still struck by the paucity of vegetable remains. Doubtless these were present in quantity sufficient to supply food to the marine animals which existed, but their perishable tissues have yielded to the elements of destruction and disappeared. The upheaved land during the *grauwacke* and *silurian* systems seems to

have been too limited to favour the production of terrestrial plants.

The disturbances which ensued after the close of the primary period, rendered the earth more adapted for vegetation. Land was upheaved, and the energetic causes then in operation must have materially assisted in effecting its disintegration. The time which elapsed between the close of the primary, and commencement of the secondary periods, would be employed in the formation of soils on this upheaved land. Soils formed from the detritus of the primary rocks would be eminently adapted for a luxuriant vegetation, such as existed during the deposition of the carboniferous strata.

But no means of removing the excess of carbonic acid of the air having been yet in operation, terrestrial plants could not be accompanied by terrestrial animals. It is obvious that this excess of carbonic acid could not be very detrimental to the life of marine animals; because the sea, saturated as it is with salts, can only hold a certain quantity in solution. But still the sea also must have contained considerably more of this gas then, than at the present day. The scantiness of vegetation was the great characteristic of the primary strata; but in the coal systems this vegetation is marvellous in its extent. The principal deposits in this series are arenaceous, argillaceous, and calcareous. The calcareous deposits of the primary period, even as high as the upper silurians, occur in detached masses, forming no continuous beds, like the carboniferous, or mountain limestones. We find a total absence of land reliquæ in these calcareous beds. Add to this, that, besides the nature of their fossil remains, they afford evidences of a very tranquil and gradual deposition; and it is apparent they must be of marine origin. The excess of carbonic acid dissolved by the water from the atmosphere, would render the sea capable of retaining a large quantity of carbonate of lime in solution, and the luxuriant vegetation which covered the sea as well as the land, would constantly be abstracting this carbonic acid for the purposes of food, from the surrounding water. The carbonate of lime being thus rendered insoluble, would, be tranquilly deposited, and thus two substances being removed, which in excess are fatal to certain animals, other forms of organic life would spring into being. Here, then, we are furnished with an explanation why the limestone does not occur in continuous beds in former strata; for however great the quantity of limestone in solution might have been, it could not have been deposited, were there no means of removing the carbonic acid which retained it in solution; only in particular localities, where adventitious circumstances occasioned the expulsion of carbonic acid, the limestone would be deposited.

And now we come to the consideration of those vast deposits of coal which form such striking monuments of a primeval vegetation. The entire coal series often attains a thickness of 1,000 yards; the beds of coal occurring in it are occasionally three or four feet thick, and not unfrequently several yards. The thickness of all the coal

beds taken together may average forty or fifty feet in the English and Scotch coal fields. Now when we consider the vast area covered by the coal series, we must feel convinced that during its formation, peculiar causes were in operation which occasioned a great luxuriance in vegetation. It is true that such rivers as the Oronoko and Mississippi roll down to the ocean vast quantities of vegetable matter ; but great as these are, they do not even furnish us with a faint conception of the manner in which the great carboniferous deposits have been formed. The wonderful luxuriance of vegetation during the carboniferous era is doubtless attributable to the amount of carbonic acid in the air. The remains of plants which constitute the various seams of coal, shew that they were principally terrestrial. Many of these beds of coal appear to have been formed of drift vegetation, but others shew every evidence of the plants having lived and died on the spot : this is the case with the North of England coal field, and most of the North American coal fields shew similar evidences ; the Devonshire coal fields, or culm measures are, on the other hand, I believe, frequently composed of drift matter.

Now in these coal fields which show evidences of having been formed *in situ*, a bed of fire clay is almost invariably found immediately below the coal. In this are present large quantities of stems and leaves of *stigmara*, *ficoides*, &c. The constant occurrence of this underclay, evidently indicates some general cause. Now, when we examine the composition of the ashes of coal, and that of the fire clay, we discover the same ingredients in both. Potash, and magnesia are contained in the fire clay, and from their constant presence in coal appear to have been indispensable to the development of the plants constituting it.

This underclay may then be viewed, with every probability, as the soil in which the plants grew ; and the adaptation of such a soil to the plants was obviously due to its alkaline constituents. These primeval plants would not exhaust a soil so rapidly as those of the present day, for they invariably contain a much smaller quantity of inorganic ingredients ; and their roots being but imperfectly developed would not furnish much excrementitious matter to the soil. Once admit that an excess of carbonic acid was in the air during this period, and an immense vegetation would be the result. The carbonic acid being once extracted could only be returned to the atmosphere by a complete decay of the plants which had used it as food. But we find that the plants constituting coal have been subjected only to partial decay. They have yielded up most of their oxygen, but their carbon has been for the most part retained : their hydrogen has also in a great measure disappeared. From the composition of coal compared with that of woody fibre, it is obvious that during the formation of 353 cubic feet of Newcastle splint coal, the atmosphere must have received 800 cubic feet of oxygen gas, and lost a corresponding quantity of carbonic acid. Now, suppose

we were to calculate the quantity of carbon in all the carboniferous deposits at two thousand billion pounds (a quantity which must be much under the truth); then during its formation no less than 64,000,000,000,000,000 cubic feet of carbonic acid must have been extracted from the atmosphere, and a like quantity of oxygen gas returned to it.* This is equal to $\frac{2}{3}$ of the quantity of carbonic acid present in the whole extent of the atmosphere. And when we consider that this is but a portion of the carbonic acid removed, we may reasonably conclude that the atmosphere contained, at the commencement of the great carboniferous epoch, more than double the quantity of carbonic acid which it does now.

We have no grounds for affirming that there is a less vegetation now than in early times. On the contrary, it is highly probable that the vegetation now is much greater than that of former periods; but it is no less certain that the vegetation of former times was *vastly more luxuriant at given places*. For the continents being of smaller dimensions, more carbonic acid could be spared to support a luxuriant vegetation in a confined area. It is owing to this great luxuriance of vegetation, within a limited district, that vegetable remains were accumulated in such quantity as to defy even a remote analogy at the present day.

Without reference to geological epochs, I may here state in what manner coal and lignites are produced. The two principal kinds of coal are, the wood, or brown coal of Germany, and the stone, or mineral coal so abundantly found in our own country.

The wood coal, from its composition, has evidently been formed by a regular decay of plants with limited access of air. Hence the hydrogen is still present, whilst the oxygen has disappeared along with carbonic acid. Mineral coal, on the other hand, is distinguished from wood coal by containing a very small portion of hydrogen. Wood coal has been formed by the evolution of carbonic acid from the sub-

* 1000 lbs of charcoal in burning produce above 32,000 cubic feet of carbonic acid. $1000 = 32,000$, or $1 = 32 : 2,000,000,000,000,000$ lbs. will produce 64,000,000,000,000,000 cubic feet.

$$\frac{2,000,000,000,000,000 \text{ lbs.}}{2240 \text{ lbs.}} = 892,857,142,857 \text{ tons.}$$

But the assumed number, 2,000,000,000,000,000, is empirical, and we have, therefore, to shew it is not *above* the truth, however far it may be below it. Now we have already seen that Manchester by fuel, for *domestic purposes alone*, sends into the air, every year, 23,614,285,714 cubic feet of carbonic acid. And, taking its manufactories into the calculation, we may safely suppose, that the total amount will not be less than 46,000,000,000. Now—

$$\frac{64,000,000,000,000,000}{46,000,000,000} = 1,391,304$$

That is, Manchester would consume the total amount we have supposed to exist in 1,391,304 years. Or supposing that there were in the world about 65,000 places consuming the same amount of fuel as Manchester, the total amount of coal in the great carboniferous deposits would be consumed in about twenty-one years. But this is obviously absurd, for we know that there is a much greater supply than this. Hence our original empirical number, instead of being above, must be much under the truth.

stance of the plants composing it; whilst mineral coal has been formed from the expulsion of part of its elements in the form of combustible oils. These oils may often be procured from the coal by distillation. Heat appears to have been the cause of the expulsion of these oils. A remarkable example occurs in a quarry within a few miles of St. Andrews, between that town and Cupar. A basaltic rock which has penetrated through the carboniferous strata, forms a hill in the locality alluded to: this rock is thoroughly impregnated with coal naptha. At whatever part a fragment may be broken off, the fresh surfaces are quite humid with an imprisoned fluid, which almost instantaneously evaporates; this fluid has the smell and all the properties of coal naptha. The great difference, therefore, in the formation of wood and mineral coal is, that in the production of the former, carbonic acid is evolved; in that of the latter a hydrocarbon. Hence it is that no combustible gases exist in the mines of wood coals, whilst they abound in those of mineral.

But be this as it may, our conclusion is still the same: that during the formation of the carboniferous deposits, much carbonic acid was abstracted from, and much oxygen furnished to the atmosphere. From the very low numbers which we assumed as indicating the weight of coal in the carboniferous strata, we have seen that its conversion into carbonic acid would nearly double the quantity of that gas now in the atmosphere. But the marine plants, which probably abounded in the same proportion as the terrestrial, have left few evidences of their former existence. They are so perishable in their nature, and surrounded by an element which aids their decay, that their preservation was highly improbable. But during their decay, the carbonic acid from which they were formed, must have been given to the surrounding water; and probably entering into chemical combination with some of its materials, was not again restored to the atmosphere. From whence came all the carbonic acid in the limestones not formed by the accumulation of shells? Some of it, certainly, may have been derived from the source just mentioned. Nor are we to allow ourselves to be misled by the belief that the quantity of carbonic acid evolved from such a source, would be too small to exercise an appreciable effect. The decomposing organic matter has perceptibly affected the whole mass of the ocean in its vast extent; for all the recent analyses of sea water prove the presence of sulphuretted hydrogen—a gas only generated by the action of decomposing organic matter on salts of sulphuric acid. There are salts of lime in sea water, particularly the sulphate of lime, now this salt is very easily decomposed by a carbonate. Supposing that during the decay of the marine plants, which every analogy leads us to suppose must have existed in quantity proportional to the terrestrial, an alkaline carbonate was produced: this acting upon the sulphate of lime would occasion a precipitation of carbonate of lime, and give rise to those soluble alkaline sulphates, which we find in such quantity in sea water. By this suggested explanation, I by no

means infer that this was a general mode by which the stratified limestone was produced: the undivided limestone could not possibly have been produced in this way. But it is possible the thin layers of limestone which occasionally alternate with the shale, sandstone or coal, in the coal formation, may be due to such a cause; or might we not conceive that the bituminous limestone shale might also owe its production to this? The immense mass of undivided mountain limestone could by no possibility have been thus produced, but may have well been by the deposition from solution through the instrumentality of the causes I have formerly described. Once allow, with many geologists, that the ocean was in a heated state during a great part of the primary period, and we are furnished with another mighty means of abstracting the carbonic acid from the atmosphere. The heated waters of the ocean could not dissolve carbonic acid, but as they cooled, this gas would be absorbed from the superincumbent air; and the water which evaporated and again descended as rain, would bring down in solution large quantities both of carbonic acid and ammonia.

Taking such things as these into consideration, together with their possibility or probability, it is obvious that we have lost all data for calculating the amount of carbonic acid in the air, at the commencement of the secondary period. Allowing them even a shade of probability, we could not deny that the former atmosphere may have contained more than twenty times the amount of carbonic acid that it does at present; and admitting that it did so, we can account for the extraordinary luxuriance of the primeval vegetation and for the absence of land animals whilst that vegetation lasted.

During the period at which the carboniferous strata were deposited, neither reptiles, birds, nor mammalia appear to have existed: nor was it possible that they could have existed, were these views of the state of the atmosphere correct.

It is not my intention to detain you, nor is it my province to wander with you step by step over the various geological epochs. In our brief sojourn in the carboniferous strata, we have seen that several, possibly many causes were in operation to remove carbonic acid from the air, and consequently to fit it for the support of animal life. But let us not suppose that these causes ceased with the termination of the carboniferous era. They still operated, though in a less striking degree during all the divisions of the secondary period; but, during the deposition of the new red sandstone, they appear to have been in a great measure dormant. It is the duty of the geologist to explain what physical causes then existed, which were so unfavourable to animal and vegetable life. But the causes which acted during the carboniferous period were again revived with the oolitic system; and, accordingly, from the coal occurring in it, we draw evidences of a removal of carbonic acid from the atmosphere, and a supply of oxygen to it. But here also we are struck with the new forms of animal life which have now sprung into existence: the

saurians which began to appear in the new red sandstone, have now multiplied, and play on the shores of the oolitic land; insects inhabit the luxuriant forests which cover that land; and that most extraordinary of all created beings, the pterodactylus, or flying lizard, executes the functions for which it was designed. The sea has acquired new inhabitants: not only monstrous reptiles, but new forms of fishes—zoophyta, mollusca, and articulosa; but still we find neither birds nor mammalia. The plants of this series afford evidence sufficient of a tropical climate, but the saurian animals furnish proof yet more conclusive. You may remember the cause of animal heat, as I stated it to you in a former lecture. It is a combustion of certain unazotised ingredients of food by means of the oxygen of the air; and as a product of the combustion, carbonic acid and water are formed. The heat occasioned by the conversion of the carbon and hydrogen into carbonic acid and water in the interior of the body, must be as great as if the elements were burned in the open air.

We mentioned that it was quite possible that marine animals may have existed when terrestrial animals could not. Of course the grand object of respiration is the same in both classes of animals, viz. transformation of the food, and of particular constituents of the blood, by means of the oxygen of the air. Hence aquatic respiration differs from aerial only in that the water becomes the medium of conveying the air to the respiratory organs. In the lower classes of marine animals, the respiration is entirely cutaneous, the air not being supplied through distinct channels, but by a transudation through membranes permeable to it. As we go higher in the scale, we find a bronchial respiration, or respiration by means of lungs. In these the water holding oxygen in solution meets with a net work of veins, and aerates the blood circulating within them. The cause of animal heat being a combustion of carbon and hydrogen by means of oxygen, it is obvious that in the cold-blooded animals, the quantity of oxygen required will be much less than in the warm-blooded. Accordingly we find this to be the case. A tench lives for some time in water containing only $\frac{1}{5000}$ its bulk of oxygen, whilst river water generally contains from $\frac{1}{2}$ to 1 per cent. of this gas. Unfortunately I am not aware of any experiments which show how much carbonic acid may be in water without being detrimental to the life of marine animals; but certain it is, that the luxuriant vegetations which analogy leads us to believe must have existed in the sea at this period, would extract carbonic acid from, and furnish oxygen to the surrounding water.

The amphibians require very little oxygen for the support of their vital functions. Frogs will live for four or six hours in an atmosphere of pure hydrogen or nitrogen, which does not contain a particle of oxygen. And although the amphibians require less oxygen than the terrestrial saurians, still we find that the economy of the latter requires much less oxygen than that of higher animals. Their lungs are,

therefore, comparatively imperfect, and the two systems of circulation incomplete; for their arteries circulate a mixture of venous and arterial blood. I do not know if there are any experiments on record, of the powers of any particular saurian to live in an atmosphere deficient in oxygen and surcharged with carbonic acid, such as we suppose existed during "the age of reptiles." I hoped to have been able to show you some experiments of their power to do so; but from their general torpidity at present I have not been able to meet with any lizards to secure for this purpose. In a few weeks, however, they will be coming out of their holes, and I shall introduce them into an atmosphere of the olden times, such as their progenitors revelled in when they were masters of the world, and multiplied themselves to such an amazing extent.

All reptiles are distinguished by the small amount of food which they consume, and by their tenacity of life under very trying circumstances. The food which they do take is not of a nature to be transformed into carbonic acid; nor are their organs of respiration suited for this transformation, even it were. Hence it is, that they do not require much oxygen for the support of their respiratory functions, and that they expire so little carbonic acid. Hence it is also, that they depend upon the warmth of the air to keep up the temperature of their bodies, having no means of generating heat within. Considering then these facts, and the supposed nature of the atmosphere during this period, we discover a sufficient cause why this should have been (as Mr. Mantell aptly denominates it) "an age of reptiles;"—they did not require much oxygen, which the air could not have afforded. Their respiratory functions were not retarded by an excess of carbonic acid, which would have proved fatal to animals of a higher organization; nor did they come in collision with the plans of the intelligent Creator to remove from the atmosphere by means of organic life the excess of carbonic acid.

From these facts we see that reptiles could have had no difficulty in living in an atmosphere containing less oxygen than at present; but we find that in the oolitic period, a particular kind of quadruped existed. This seems to have been a marsupial animal allied to the didelphys. It was evidently an insectivorous animal, from the conformation of its jaw bones found in the Stonesfields oolitic beds, where the elytra of land beetles are found accompanying them. Now can we find nothing in the respiratory system of the marsupials which would lead us to believe that they might have been in an atmosphere such as we have supposed to exist? The marsupials of course breathe, like other mammals, like man himself, by the lungs alone; and if any peculiarity of the respiratory system existed in the primeval didelphys, we could scarcely expect to find anything but mere traces of it in its modern congeners, changed as they must have been to suit the varied conditions of the atmosphere; but let us try to discover whether such traces may not have been preserved.

Now in saurians, chelonians, and fishes, two canals are observed

to issue one on each side of the anus into the peritoneum, that is to the external surfaces of the viscera. Their use seems to be to carry on a partial aquatic respiration, or, in other words, to supply aerated water to the blood circulating in particular vessels. It would also appear that more highly oxygenated blood is required for those vessels which supply the brain. This adjunctive respiratory system is supposed to subserve this purpose. Its presence evidently indicates a want of oxygenation of the blood. *Now it is very remarkable that traces of these canals exist in the marsupials.* Mere traces, however, of any structure, do not subserve functions; they may be considered in two lights, either as remains of what have been, or as general indications of some of the phases through which all the parts of organized bodies pass during development. Before drawing conclusions regarding the full development of these traces in former types, we must also remember, that no class, order, family, or genus of animals or plants, ever did or ever can pass, beyond the bounds of the type after which they are formed. Again, traces do not *necessarily* indicate that a *full development* once existed in the family; it only proves, that such a family was or is made after the type of some division or other of the animal or vegetable kingdom; hence the traces may never have been fully developed at all,—they may be consequences of a *law of development*, and not active organs, or parts in the organism in which they are found. Thus in the present state of physiology, it would be rash to draw the conclusion that the traces of the canals to which we referred as now existing in marsupials, are certainly but the remains of what were fully developed in their primeval types. We incline to the idea, and suggest it to the consideration of those more conversant with such subjects; but at the same time allow, that it is not a *necessary* consequence of their existence, although it may be a *probable* one. So far, however, is certain, that it forms a kind of connecting link between the respiratory systems of marsupials and reptiles. But, in considering a matter such as this, we must not confine our views to the respiratory system alone, but must take into consideration the whole organization of the animal, and the peculiar connection of its various systems, with their modes of reaction. Viewing it in this light we find the animals in question occupying only a low position in the scale of creation; it is, therefore, highly probable that they could live in an atmosphere considerably worse than they now enjoy. Here, again, I must apologise for want of a few experiments which would have at once decided the question; but the short time which has elapsed since I was requested to prepare this lecture for you, has prevented me from procuring an opossum to treat to such an atmosphere. Any of you who may have such an animal may easily satisfy yourselves by a few experiments.

But whilst wandering through these ancient lands, how comes it that we have not met any of the winged tribes? Forests are there to form a habitation—insects abound to afford them food; the genial

climate or the smiling face of nature invites them to sing its praises ; but in vain we penetrate the deep recesses of those ancient forests to discover traces of the feathered songsters of former days. We search the shores and the rivers to find aquatic birds feasting on the fishes which so abound, but we are scared away by the saurians which line their banks. We enter the forests, and meet nought but the monstrous pterodactylus sailing along on its filmy wings. If birds existed, where are their remains ? Though rare, still we find them scattered throughout the tertiary and modern lacustrine deposits. Why is there not a single evidence of their existence in all the secondary strata ? Simply because (if our view of the state of the atmosphere be correct) they could not have existed. Birds require a very large supply of oxygen for the support of their vital functions, and are peculiarly susceptible to the effects of an excess of carbonic acid ; neither of these conditions being yet favourable, nature was not fitted to receive them.

But we have now nearly approached the termination of the secondary period, for the paucity of terrestrial plants and animals in the cretaceous series and greensand does not tempt us to linger long amidst their oceanic deposits. But to the chemist it seems strange from whence came those vast deposits of carbonate of lime in the cretaceous beds. The plants are not numerous, and almost entirely marine ; yet from the quantity of pyrites which occurs, it is apparent vast quantities of organic matter must have been in decay.

The perishable nature of marine plants prevents them being accumulated in very large quantity. Hence we might still conceive that the ocean was covered with marine plants at this period, which by their decay, might so furnish carbonic acid as to decompose the sulphate of lime existing in the sea water, and thus occasion the deposition of carbonate of lime ; thus the atmosphere would be robbed of large quantities of carbonic acid. But the magnitude of such deposits astonish us, and would compel us to relinquish this even as a partial cause, did we not, on the other hand, consider the great extent of geological epochs.

Now came the close of the secondary period. We have glanced at the nature of its organic remains, from the silurian rocks to the greensand ; we have been struck with the developement of organic life ; we found it partly owing to the physical conditions and position of land and sea—and with these we had nothing to do ;—but we found it also dependent on the chemical constitution of the atmosphere ; and that when vegetation gradually purified the air from its noxious ingredients, other forms of animal life sprung into existence. When traversing these lands of former times, it is difficult for us to conceive that we are examining our own world. The striking progression of organic life must have been due to some cause. Why, may I ask the followers of Lyell, who believe that organic beings may have existed long before the primary periods, although their remains have been destroyed by heat, why this pro-

gression of animal life? And, wherefore, may I ask the followers of Daubeny, did land animals not sport in the forests of the carboniferous land? No mighty operations of nature then acted as antagonists to their existence; the climate must have been congenial; the war of the elements not greater, if so great as now. And wherefore, when in the course of ages, those forests became entombed, did land animals then start into being? Can these, or many similar questions be replied to, without admitting some changes in the states of the medium in which both plants and animals exist, or can the uniform progression of animal life be attributed to entirely local or adventitious causes? Let us not contort the face of nature to give countenance to our theories, but let us frame those theories on nature as it is.

Following, therefore, the train of reasoning which we have adopted, we are struck with amazement in stepping over the boundary which separates the secondary from the tertiary periods. We no longer encounter those monstrous reptiles which haunted the shores and rivers of former lands; we do not now wonder at the vast difference of the former state of things, but feel a difficulty in believing we are not looking upon a world similar to our own. We have seen that the forms of animal life either gradually run into one another in the various strata, or that these were separated by a distinct line of demarcation, and characterised their own peculiar class of fossils. With respect to land animals, we have also seen that when any great addition of forms was effected, evidences exist of causes having been in operation to remove carbonic acid from the atmosphere. Now whence do we derive the evidences of this withdrawal in the era between the secondary and the tertiary periods? Nothing can be more striking than the difference of organic remains between the *infra*, and *supracretaceous* beds. The *supracretaceous* deposits are characterised by the wonderful similitude of organic remains to types now existing—the *infracretaceous*, by their utter dissimilarity. On this account we are disposed to lay more weight to our former explanation of the manner in which the *cretaceous* beds may have been formed; viz., by a decomposition of the sulphate of lime existing in sea water by means of carbonates formed through the agency of decaying plants. Such, at all events, may have been a partial cause, and would account, in a great measure, for the small coherence and chalky nature of the limestone; a character of a precipitate.

There are many circumstances which countenance this idea. The recent experiments of Kuhlman on the preparation of artificial stones by means of silicate of potash, are powerful advocates of its truth. There cannot be the slightest doubt that silicate of potash formed an important ingredient in the sea of former times; every rock of marine origin proves this. Now as the carbonic acid was evolved by the decay of the marine vegetation, it would decompose this silicate of potash, forming carbonate of potash, and depositing

silica. This carbonate of potash again meeting with the sulphate of lime in solution, would occasion the double decomposition of which we have spoken. And that these chemical changes did take place, the composition of the chalk shews us; for it every where contains both potash and silica. The organic remains converted into silica owe their origin to a like cause; that is, to a gradual deposition of silica, from the silicate of potash decomposed by means of the carbonic acid evolved during their decay. The infiltration and decomposition of this salt would then give the coherence to the chalky beds which they now possess. Indeed, to the infiltration of silicate of potash may be ascribed the conversion into stone of all the argillaceous and calcareous deposits; assisted, of course, by heat.

I am well aware that the supposition which we have advanced has to encounter a very serious objection in the paucity of vegetable remains, found in these beds; but this objection is by no means conclusive against the opinion. The plants found in them are all marine, and from their perishable nature could not be accumulated in quantity. Besides, the loose nature of the deposit would long admit of the access of water, which would ensure their decomposition. This circumstance must be allowed its due importance; for the impermeability to water in chalky beds is vastly inferior to that of arenaceous and argillaceous deposits; and the experiments of Dr. Lindley have proved that long immersion in water destroys most plants. Allow, then, even this suggestion as a partial cause (but we see no obstacle in assuming it as the universal one), and there is no difficulty in accounting for the withdrawal of carbonic acid from the atmosphere during the deposition of the cretaceous beds. In consequence of this withdrawal, the atmosphere becomes fitted to sustain higher forms of organic life, and these accordingly sprung into being during the tertiary period. During the tertiary period itself, this withdrawal of carbonic acid from, and supply of oxygen to, the atmosphere, was constantly proceeding, as the vast beds of lignite on the Rhine amply testify; and a gradual increase of forms of animal life is accordingly observed, from the basis of the tertiary rocks, to the summit of the series. It would be desirable that more conclusive evidences were furnished of the age of the brown coal: in absence of information to determine to what part of the series it should be placed, we must keep from drawing conclusions regarding it; the general inference, however, may be drawn, that these entombed lignites do certainly not belong to the newer part of the series.

Although we find very many species in the tertiary strata similar or identical with those now inhabiting the earth, still neither in these nor in the post tertiary or alluvial deposits, do we find any trace of man or of his works. But previous to his creation it became indispensable that a balance should be established between animal and vegetable life. It was the commencement of a grand era that ushered into the world an intellectual being. Nor can we conceive that a creature so

noble, stamped by the image of its Creator, could be destined, like those animals which had become extinct, to be swept in its turn from the earth by causes similar to those which had effected their extinction. The creation of an intellectual being was not an occurrence coming in the common order of events; but man must have been placed upon the earth by a fiat of the All-powerful Creator. The present order of the globe was ordained for him, and all things proceeded to fit it for his habitation. We will not deny that he who made man might likewise ordain that he should be swept from the earth; but we do deny that the exhibitions of the causes destined to produce this would be in uniformity with the usual designs of providence. It would be ill-accordant with a divine wisdom to suppose that it had not arrested the causes which led to the extinction of whole tribes of animals, when it called man into being. But it would as little harmonize with our conceptions of the Creator's works to imagine that any divine interposition or miracle altered the face of nature, immediately antecedent to the creation of that intellectual being. Laws were instituted, by which this earth is governed, and we must look for such changes in the natural current of their operation, not in their annihilation or alteration. Now if I have carried you along with me in the description of events which have proceeded on the earth from the first dawn of organic life, you will find no difficulty in discovering, or admitting with me, that the grand causes for the extinction of animal life were now removed. We have seen all the primeval lands, which we have hastily traversed, covered with a luxuriant vegetation, but containing a disproportionably small number of forms of animal life. We have approached more nearly to our own epoch, and remarked the gradual increase of these forms; and, at the same time, we have seen that the atmosphere which covered those lands, gradually changed its character. We have remarked that the first forms of land animals were such as could live in an atmosphere destitute of its present proportion of oxygen, and such also as did not return any notable quantity of carbonic acid to the atmosphere. But we remarked also a great change as we approached our era:—the animals existing in strata antecedent to modern deposits, were all furnished with respiratory organs like our own; that is, with organs fitted for abstracting oxygen from the air, and returning carbonic acid: functions quite opposed to those of vegetables, which abstract carbonic acid and return oxygen. Hence, as soon as animals became sufficiently multiplied to supply the amount of carbonic acid to the air, equivalent to the quantity abstracted by growing plants, a balance would be struck, and the air could not experience any appreciable variation. For even supposing that by some adventitious circumstances, such as by volcanic agencies, an increased supply of carbonic acid was furnished to the atmosphere, the effect would simply be to induce an increased vegetation, and the excess would be withdrawn. On the other hand, if the amount of carbonic acid became

diminished, vegetation would be retarded, and it would attain its normal standard. Such would be the natural effects of vegetation, when the original amount of carbonic acid in the air had become reduced to the point at which it ceased being detrimental to animals, but was still sufficient to administer to an ordinary, but not excessive luxuriance of vegetation.

But here a curious question arises—Does the progress of human society not occasion a greater demand for carbonic acid, and thus serve to destroy the equilibrium between plants and animals? It can scarcely be denied that civilization causes an increase in the aggregate of animal life. The preponderance is great between the animals depending upon man, and those destroyed by him. Nay, even the increase of human beings produced by an augmentation of the comforts of civilized life, would demand our serious attention to the enquiry. Once admit, and I do admit it for the sake of argument and probability, that civilization increases the aggregate of animal life, and it is evident we must find some means of compensating to the air for the carbon and nitrogen abstracted and retained by this increase. For were there no means of compensation, other parts of the earth, existing in a state of nature, would suffer for the supply of those parts subject to the dominion of man. The difficulty of finding an answer to this question has furnished a specious argument to those who refuse to admit that the food of plants is derived from animals; and yet the explanation is not so difficult. Which would require the most carbonic acid—America with its forests and extensive prairies, or America peopled by man, and covered by fields of waving corn? Civilised man enters America, he expels its tenants from their native soil, cuts down their forests, and plants in their stead agricultural produce. Strange as it may appear, he has caused neither an increase nor a diminution in the amount required; for it has been proved by Liebig that the same extent of land produces the same quantity of carbon, whether it be covered with forests or agricultural produce. He increases himself to an amazing extent, and thickly peoples that once thinly populated country. True, by his increase, he removes from the air quantities of carbonic acid, but he compensates for this by the habits of civilization. He has cultivated in the place of forests plants requiring a large supply of nitrogen. This nitrogen he is peculiarly fitted to supply them with, from the effete matter discharged by him. The air contains much more ammonia than is necessary for the purposes of wild plants; he abstracts out of its superabundance, and merely concentrates it at particular spots, where it meets with plants which peculiarly require it, but none of it is lost to vegetation. The peculiar habits of civilised life cause him to return to the air carbonic acid, amply compensating for that abstracted by his increase. In the form of coal he digs from the earth, and returns to the air the carbonic acid of former times. The great stream of air which, by the revolution

of the earth moves from the equator to the poles, wafts in its return this food to tropical climates, where nature yet revels in all her wildness. The food thus sent is immediately appropriated by a luxuriant vegetation, and oxygen of course emitted, which the same stream of air in its progress towards us, brings back to supply that consumed in the formation of the food. Thus man, by the habits of civilisation, fully compensates for that which he retains by his own increase, and that of the animals dependant upon him. Upon the compensation of ammonia, I have almost said enough. The plants of former ages were not such as to require much nitrogen; hence, although the decrease of carbonic acid from the air was great, that of ammonia was insignificant. The greatest exhausting cause in operation would be the rain which carried it to the sea, and caused it to remain there; that falling on the land would again evaporate. And though the increase of animal life may, as we have already said, remove for a time the superabundance, this will finally benefit vegetation. But all this time, I have been silent regarding volcanic agencies. I have been so, because I consider them merely as an auxiliary means of furnishing to the air the carbonic and oxygen retained by animals during life; but by no means as being the primary sources of food.

I would I could dwell longer upon these subjects: they are of too extensive a nature to be embraced in a single lecture; but for you I have been already far too long. My end will be gained if some of you have been convinced that the grand causes formerly in operation for the destruction of animal life have ceased, and that man is not liable to the destructive agencies of former times—if you are convinced that by wonderfully wise plans of Providence, the grand medium of animal and vegetable life, the atmosphere, has become fitted for the reception of man, and attained a state of repose and perfect equilibrium, by the exquisite adjustment that *Death* has become the source of *Life*.—And if my illustrious namesake, when supporting the theories of Hutton, in his assertion that the world shewed no traces of a beginning nor none of an end—meant to include that the organic life on it is also destitute of such traces, we cannot assent with him. But that no evidences of a beginning nor of an end exist in the world itself, we fully admit; yet that that beginning was, and that an end will be, we have the word of one “that cannot lie.”

Atmospheric Electricity.—By Mr. T. SPENCER.

SECTION IV.

Liverpool Albion, Feb. 14, 1842.

I shall occupy a portion of the present section by explaining the electrical phenomena attendant on hail storms, taking the undisputed fact, as already granted, namely, that hail and pieces of atmospheric

ice are formed from the water contained in cloud : and furthermore, the position already assumed in the present papers, that cloud, as a necessary condition of its existence, possesses a definite portion of electricity. The explanations that follow, arise inductively, out of these points. The brief remarks I shall make on this head will have no relation to the formation of the hail itself, but only to the attendant electrical phenomena, together with the whiteness assumed by hail and atmospheric ice, both of which are, as yet, unattended with any satisfactory explanations.

All observers have been particular in remarking, that violent hail-storms are attended by lightning, and often by thunder ; but whether these phenomena are the cause, or only an effect, produced by these occurrences, is I believe, undetermined.

Another important fact has also been noted, that the lightning given out is seldom so intense or dangerous in its effects as that given out during ordinary thunder storms, accompanied with rain, although there are rare exceptions to this rule ; but the rule and exception are, I submit, easily explained according to the views already given in former portions of this paper.

Let us here, however, in some degree, recapitulate. Aqueous vapour possesses upwards of 1200° Fah. of latent caloric. It possesses this amount as it floats in an invisible state at all times in our atmosphere. When condensed into globules of water, in which state it is true vapour, to form cloud, it parts with 1072° of caloric. Still, it will be observed, there is an important quantity yet held latent, amounting to 140° Fah., which latter portion is termed the caloric of fluidity, or, in other words, that portion of caloric that is necessary to the water, in order that it may maintain its fluidity, *or that which prevents it from becoming ice.*

We have already seen, that transparent aqueous vapour, or gas, in becoming water, *it being a chemical change*, develops, simultaneously with its latent caloric, a definite portion of electricity, obeying the law of all chemical change.

Bearing, then, this in view, and proceeding but one step farther, we at once perceive the necessary sequence of the electrical phenomena that accompany hailstorms.

When a cloud is deprived of its electricity, no matter by what cause, as already stated, the small globules of water condense into large ones, and form rain. Under ordinary circumstances, this rain would be carried to the earth in the fluid state ; but when hail is formed, the rain, instead of descending, is carried upwards by an up-moving current of atmosphere, as we have already seen explained, and losing, as we know it does, 1° of caloric for every one hundred yards it ascends from the level of the ocean, it soon arrives at a temperature sufficiently low to deprive it of its caloric of fluidity. When this takes place, the globules of rain are at once converted into ice. When, however, the 140° of caloric of fluidity is eliminated, if (as before laid down) it is true that electricity is developed in the free

state, when the 1072° of caloric of elasticity is liberated, the same phenomena *must* attend the formation of the drops of rain into the pieces of ice, termed hail, that attends it in the former instance; or, in other words, the electricity must be given out simultaneously with the caloric of fluidity; but, it must be borne in mind, in *less degree* than when the caloric of electricity is developed.

This fully accounts for the flashes of lightning that accompany hail and storms. The lightning, as previously indicated, is harmless, comparatively, and necessarily so from the small quantity given out. The fact of it being diffused through an immense space also renders it of exceedingly low tension.

When speaking of this and other atmospheric phenomena, especially those related to or accompanied with electricity, an inhabitant of our own climate is somewhat prone to imagine that the meteorologist exaggerates the importance of these appearances. He at the same time finds it difficult to give credence to what he hears related of their effects; inasmuch as he is unacquainted with any, or but rare and mild examples in his own climate.

This may apply not inaptly to the lightning and intense violence of hailstorms; but an inhabitant of a mountainous district in south or central Europe has no such difficulties, as he witnesses them but too often during the summer and autumn.

Connected with this subject, it may not be uninteresting to show how practice has preceded theoretical discovery.

In the south of France and in parts of Switzerland, it is not unusual for proprietors of vineyards to elevate rods of wire to a height of from twenty to thirty feet in different parts of the grounds. These are termed *paragrêles*, and are erected with a view to protect the crops from those sudden showers of hail and pieces of ice that occasionally destroy the vintage of whole seasons. I am informed they are attended with the best effects, and if so the rationale of their use must be as follows:—Primarily the storm cloud that settles or forms over the vineyard descends in fertilizing rain, which, without the precautionary paragres, would have discharged itself in hail, breaking down and destroying the fragile vegetation.

The reason of this appears to be obvious. The elevated rods discharging the cloud of a portion of its electricity, the water is, in consequence, precipitated in the form of rain; but, were this discharge of electricity not to take place at this period, the repellant globules would be carried perpendicularly up by the current caused by the formation of the storm clouds until they come to a cooler region of the atmosphere, there to be condensed into hail, where they would be thrown sharply off at the annulus of the upheaving current, accompanied by a flash of lightning, and, according to the circumstances of the next cloud, thunder. The hail would now be precipitated from a great height to the earth, and, from its force, would destroy the tender and fragile vegetation.

In these instances, the flash of lightning is given off from the

necessary decrease of surface that takes place when the small drops of water are condensed into the large drops that form the hail. So much is this method of producing rain instead of hail appreciated in those countries, it is stated, that, in the sandy arid plains of the south of France, the French Academy have instructed the proprietors of these districts to elevate rods of wire, at intervals, with the expressed object of attracting the clouds to discharge their rain; and, if the principles I have laid down are correct, it will, most likely, be attended with the desired effect.

It often happens, however, that hail falls in pieces of ice of immense magnitude, as if sheets of this substance were suddenly discharged and broken into pieces. This phenomenon is caused by the sudden abstraction of the electricity surrounding the globules of water, in its passage upwards, before it arrives at a temperature less than 32° Fahrenheit. The consequence of this is, that the globules condense into sheets or streams of water, which, in that state, are still borne upwards, and thrown off at the annulus of the upheaving current, where, meeting with colder air, they are suddenly frozen, and are precipitated to the earth in irregular masses. A hailstorm of this description is preceded invariably with thunder and lightning, and it is difficult to see how it could be otherwise.

We now arrive at the cause of the whiteness of snow, hail, and atmospheric ice, as distinguished from the general colour of ice frozen on the surface of water.

First, snow is cloud frozen at a high comparative altitude, each respective globule of which possessing its atmosphere of free electricity. These globules, while in the state of water, in all probability do not possess those beautiful arrangements that snow is found to possess, when viewed under the microscope, nor can they be so far apart.

The crystalline or radiant form proceed from the secondary electricity given out, as we have seen, during congelation. That this must be slowly and gradually developed, we may gather from the regularity of the forms. We have familiar instances of this in the laboratory, as we observe during the aggregation of crystalline masses on which free electricity is brought to bear: should there be a large quantity of electricity present, the forms will assume a friable, irregular appearance; but where it is slowly developed, and of feeble tension, the form of the body is the more compact and regular.

But, on the other hand, with hail, the drops of rain are already formed, and carried up with the current, where they become suddenly frozen *as they are*. Electricity is, in this, as in the other instance, given out, and prevents the particles of nascent ice from aggregating themselves as closely as if the same phenomenon took place on the surface of water, because the moment it was set at liberty it would be conducted away; but in the upper regions of the atmosphere there is no such conducting substance in contact with it.

The pieces of ice that descend are white from the same reason. We have a familiar instance of this on the earth's surface. We often observe white ice and black ice; but it is worthy of remark, that where the white ice is seen there is generally no water underneath, it having all been frozen, consequently there was no ready conductor, and the congelation took place in conjunction with a portion of the electricity that was liberated. On the other hand, in deep water, we see black, dense ice, because the electricity liberated is quickly carried off by the fluid water.

Having thus run cursorily over the most prominent parts of the views I have to propound on meteorological electricity and the attendant phenomena, I shall now briefly adduce the grounds on which my opinions have been formed.

Most chemical observers are agreed in opinion, that all chemical change is attended with a development of heat and electricity. Indeed, so much has this been observed, of late years, by the experiments of Faraday having called attention to it, that it may fairly be said to be an admitted law of nature.

I am a firm believer in such a law, because I find it coincident with all the experiments I have made, even should I have paid no credence to those made by others, which I am very far from doing. I have applied the knowledge of this fact to the solution of some meteorological phenomena.

First, then, I have viewed the transparent aqueous vapour that in all cases pervades the atmosphere as a gas. As this has been disputed, I may briefly state my reasons for adopting this view. When we classify bodies, whether compound or elementary, the segregation is made, in accordance with the properties they may possess in common, as the elements we term metals, generically, from platinum down to potassium, have certain properties in common: although, looking at these substances by themselves, scarcely two bodies in nature can be more dissimilar, yet we agree, and properly so, to term them metals. In like manner, then, with respect to the substance we term steam, when locked up in the boiler of the engine, and transparent aqueous vapour, when the same substance is in an invisible state in the atmosphere, I think I am correct in classing it under the head of a gas, for the simple reason, that it, too, possesses properties in common with gaseous bodies. Let us now see what these properties are. It is highly elastic: so are gases. It has a low, specific gravity: so have the gases; even lower than some gaseous bodies. It floats in an *invisible* state in the atmosphere: so do all the gas of which we already admit the atmosphere to be composed. It obeys a peculiar law which we already know all gases do,—the law of gaseous diffusion. Need I say more? I may add, that most of the continental chemists I am acquainted with view it in the same light, and some in our own country. Nay, the matter appears so obvious that it seems a work of supererogation to say more on this head.

This gas, then, is a compound ; but, *so far as we know*, all the gases are *not* compounds. Some of them we term simple or elementary ; still, we have compound gases, as carbonic acid gas, and many others. This body, then, we must look on as such a gas, namely, a compound gas, made up of water and caloric, to say nothing of electricity ; and, like the compound gases I have named, the combination is a chemical one. Let us trace the analogy that exists between it and carbonic acid gas. Carbon is united with oxygen to form this body : both of which, *singly*, play a distinguished part in the phenomenon of combustion ; yet, when combined into one body, it would be difficult to name anything in nature to which it is a greater enemy. This resultant property, we say arises from the bodies having entered into combination chemically, because the opaque black body carbon, is transparent ; and the same eminent supporter of combustion, oxygen, will put out a candle ; and this is the correct way of viewing it.

We now come to the aqueous gas, *steam*, composed, as far as we know, of water and caloric. Water, one of the most elastic or incompressible of bodies, having a specific gravity more than some metals, and which binds it to the earth, yet, when it combines with a definite (*mark, like all other chemical compounds*) amount of caloric, it at once, as if by magic, loses these properties and assumes new ones. From being one of the most unelastic of bodies, it becomes, perhaps, the most elastic. From a heavy body it becomes, as we have seen, really lighter than the air we breathe, in the ratio of 640 to 1000, and mounts high into the upper regions of the atmosphere. While water, it moistened all bodies it came into contact with ; but it even loses that property, and in time of drought, the air is surcharged with it to an incredible degree. On the other hand, the caloric contained in the same body, is so combined with it as to be perfectly insensible ; that is, it will heat no body that it comes into contact with, although it contains, in reality, as much, or, indeed, more heat, than would render *iron visibly red hot during the light of day*.

I might reasonably pause here and ask, need I adduce further proof of a *strictly chemical change* having taken place ?

Now, as no human eye has ever observed, nor, in all probability, ever will observe, chemical action, properly speaking, take place, it may be asked how we know that it has taken place at all ? I answer, that it is known to take place simply by the effects produced : those effects are known, as I have often repeated, when the bodies, entering into simple combination form a third, having properties totally distinct from either. Could I, then, have adduced an instance within the whole range of natural substances that more completely answers all the conditions of chemical change than the production of aqueous gas from the two bodies, water and caloric ? I, without hesitation, answer no. But here, after all, is the stumbling-block with many who cannot imagine by possibility that

caloric has an entity, or rather is a *body*. If they could be made to comprehend this, then the difficulty ceases. As for myself, I firmly believe it to be a body. I can comprehend it, and, if asked how I know this, I answer, by its effects, just as I have already stated we know when a chemical change has taken place by the effects merely. Those who, at first, pronounced the air we breathe to be a body possessing substance were considered visionary, although we now know that the whole of the gases contained in it combine with other substances to form really solid bodies.

I have assumed, then, and I am not alone in the assumption, that when the change takes place that liberates the two bodies, water and caloric, it is strictly a chemical one, and, as such, obeys all the conditions we know are attendant on such changes. One of these is, that a definite amount of electricity is given out.

The electricity when thus set free, not having the tendency to expand or rarefy the air, that the caloric has that is liberated at the same time, immediately attaches itself to the next conducting surface, which being the globules of nascent water, each globule then becomes surrounded with an atmosphere of electricity. The attraction between these two bodies prevents the water from falling to the earth, which it inevitably would, were it deprived of the electricity. Globules of water, then, and a portion of free or positive electricity, constitute the body that floats in our atmosphere termed cloud; but deprive it of the latter, and as cloud, it is annihilated, as the water will at once descend to the earth.

Here, then, is the pivot on which the whole of the observations I have made on the subject of atmospheric electricity turns. The whole of the arguments I have adduced have been built on this one fact. Take it away, and the totality of the structure I have endeavoured to rear it on, however elaborate or ingeniously contrived, or however plausible the details may be, at once tumbles to the earth—as it ought.

But if, on the other hand, it does appear, by the test of all subsequent experiment and observation, that I am borne out in this view of the *necessary* nature of cloud, I then contend that the explanations I have endeavoured to make known are the just, I would say inevitable, deductions that arise out of a knowledge of this law of nature.

NOTE.—A fifth section will be devoted, to explain several of the facts on which these observations have been founded.

*Meteorology. Observations on Shooting Stars, made at Paris, by M. E. BOUVARD, from October 1840 to October 1841.**

During a period of twenty years, many skilful philosophers have made observations on shooting stars, which have conducted

* Comptes Rendu, No. 22, Nov., 1841.

them to very curious consequences. M. Brandes, in 1823, was enabled to determine the height of a certain number of shooting stars, by observing them simultaneously with persons situated in different places. M. Quetelet, and other philosophers, have also been much occupied in observing these phenomena. M. Arago, in an article inserted in the *Annuaire* of 1836, has summed up with great care the most certain observations which we have on this subject, and has said, among other things, that the most habitual direction of shooting stars seems to be diametrically opposite to the movement of the translation of the earth in its orbit, but that it would be desirable that the result of M. Brande should be established on the discussion of a great number of observations. This question, then, is far from being cleared up; it is this fact which has induced me to attempt a regular system of observations. I here present a summary of the first year. If the results are not so certain as I could have desired, it is because the observations are not yet sufficiently numerous.

I have confined myself as much as possible to observations from between eleven o'clock at night to one o'clock in the morning, whenever the weather was fine, and there was no moon. I always made my observations facing from east to north-east. I will now give my mode of proceeding in my observations. Immediately on observing the appearance of a shooting star, I marked with great care, in short, the hour, the magnitude of the star, the colour, the duration of its appearance, and the arc apparently travelled over; I traced at the same time on a celestial globe placed before me the direction and position in relation to stars and constellations. I took no longer in reporting these particular circumstances than the phenomenon was in presenting itself; the time employed in these annotations was rather more than a quarter of a minute's duration. After each series, I indicated the general appearance of the sky, the temperature, the height of the barometer, the direction of the wind. In transcribing all these observations into a register, I took care to determine the intersections of the ecliptic with the directions of stars directly prolonged, by applying on a celestial globe a metallic semi-circle passing by the extremities of the routes travelled over. I give also the apparent directions relative to the trace of a small circle parallel to the meridian, and passing by the middle of the apparent direction observed; to determine them I have taken account of the hours of observation and of the position of the heavens.

The summary of the observations which I present to-day comprehends a year, from the month of October, 1840, to the month of October, 1841, exclusive. In this interval I have been able to observe 572 shooting stars in 86 days. I may remark that it has not been possible to prove the extraordinary appearances of November, 1840, and August, 1841. The principal results will be found in the following tables.

TABLE I.

MONTH.	Num. of Days of Obsr.	Dura- tion of the Obsrv.	No. of Shoot- ing Stars Obsr.	The Mean Intrvl. betwn. each Appear- ance.	Duration of the Appearance.						
					0 to 1s.	1 to 2s.	2 to 3s.	3 to 4s.	4 to 5s.	5 to 6s.	6 to 7s.
		h. m.		m.							
Oct., 1840..	5	5.14	30	10.47	4	13	10	5	2	0	0
November ..	8	7.17	70	6.24	13	33	14	6	2	0	1
December...	7	4.32	23	11.83	6	13	2	0	1	1	0
Jan., 1841..	6	5. 1	27	11.15	10	11	1	2	1	1	0
February ..	5	4.35	21	13.10	8	9	1	3	0	0	0
March	14	11.16	55	12.29	33	18	3	1	0	0	0
April	8	6.42	43	9.35	28	8	0	2	1	0	0
May	5	4.21	24	10.87	19	3	1	1	0	0	0
June	6	6. 3	40	9.08	28	8	3	0	1	0	0
July	5	5. 9	72	4.30	57	11	3	0	0	0	0
August	6	4.50	69	3.91	49	9	1	0	0	0	0
September ..	11	9.22	98	5.74	71	18	4	3	1	0	0
TOTALS...	86	74.22	572	7.80	326	154	43	23	9	2	1

TABLE II.

MONTH.	Directions prolonged relatively to the cardinal points, whether it be of the direction or the movement.							
	S. to N. and N. to S.	SSW. to NNE. and NNE. to SSW.	SW. to NE. and NE. to SW.	WSW. to EWE. and EWE. to WSW.	W. to E. and E. to W.	WNW. to ENE. and ENE. to WNW.	NW. to SE. and SE. to NW.	NNW. to SSE. and SSE. to NNW.
October, 1840	2	7	11	6	4	2	4	3
November.....	6	18	14	7	6	3	22	4
December.....	3	2	5	1	3	4	3	2
January, 1841.....	2	1	6	6	3	1	8	0
February	0	3	6	3	4	0	3	1
March	3	4	10	3	6	5	18	6
April.....	0	5	12	4	4	1	12	4
May	1	1	2	1	5	1	10	1
June	3	5	10	2	7	2	10	2
July.....	4	9	18	0	11	5	19	6
August	5	2	6	1	5	3	32	5
September.....	6	10	24	3	12	3	33	5
TOTALS.....	35	57	124	37	70	30	179	39

TABLE III.

MONTH.	Intersection of the Ecliptic by the direction of the shooting stars, whether of direction or movement.						Long. of the Earth, corresponding to the Mean of the Observns.
	Aries and Libra.	Taurus and Scorpio.	Gemini and Sagittarius.	Cancer and Capricorn.	Leo and Aquarius.	Virgo and Places.	
October, 1840	3	5	14	15	1	1	35°
November	3	11	11	19	18	8	60
December....	2	2	4	5	7	3	90
January, 1841.	5	3	5	8	4	2	121
February.....	5	3	0	3	4	5	148
March.....	12	10	7	8	6	12	179
April.....	11	10	7	5	1	9	211
May.....	1	5	8	4	1	6	238
June.....	7	7	8	8	5	5	265
July.....	15	11	9	8	13	15	300
August.....	16	25	6	3	6	3	328
September....	19	19	21	10	12	17	353
TOTALS..	99	111	100	96	78	80	

We cannot draw any certain consequences from all the results which I have just presented: there are too few observations to make the actual numbers be the faithful representation of the truth. However, on examining the second table, we see that of the number of stars observed, the 0.31 hundredths moved in the plane which passes by the N. W., and S. E., and 0.22 in that which traverses the S. W. and the N. E.; whilst that the other directions are in the most feeble proportions.

In considering the latter table, it seems that the most general direction of falling stars appeared diametrically opposed to the movement of the translation of the earth, as M. Arago has observed in his instructions for the exploring ship *Bonite*. Thus, in the month of October, the maximum of the apparent directions, is formed in the planes which pass by the signs Gemini and Sagittarius, of Cancer and Capricorn; in the month of November, this maximum advanced to 30°, a movement equal to that of the earth. There are many months which present anomalies, but the cause proceeds no doubt, from the too small number of stars observed. I do not attach then to these results more importance than my observations allow of; I have merely thought that it was not out of place to make the remark, precisely because there is a certain agreement with that which M. M. Brandes and Arago have said.

On the Heat evolved by Metallic Conductors of Electricity, and in the Cells of a Battery during Electrolysis. By JAMES Prescott JOULE, Esq.*

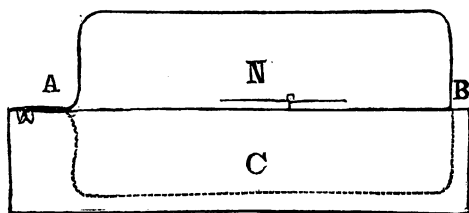
1. There are few facts in science more interesting than those which establish a connexion between heat and electricity. Their value, indeed, cannot be estimated rightly, until we obtain a complete knowledge of the grand agents upon which they shed so much light. I have hoped, therefore, that the results of my careful investigation on the heat produced by voltaic action, are of sufficient interest to justify me in laying them before the Royal Society.

CHAP. I.—*Heat evolved by Metallic Conductors.*

2. It is well known that the facility with which a metallic wire is heated by the voltaic current is in inverse proportion to its conducting power, and it is generally believed that this proportion is exact; nevertheless I wished to ascertain the fact for my own satisfaction, and especially as it was of the utmost importance to know whether resistance to conduction is the *sole* cause of the heating effects. The detail, therefore, of some experiments confirmatory of the law, in addition to those already recorded in the pages of science, will not, I hope, be deemed superfluous.

3. It was absolutely essential to work with a *galvanometer*, the indications of which could be depended upon, as marking definite quantities of electricity. I bent a rod of copper into the shape of

Fig. 1.



a rectangle (A B, fig. 1), twelve inches long, and six inches broad. This I secured in a vertical position by means of the block of wood C; N is the magnetic needle, $3\frac{3}{4}$ inches long, pointed at its extremities, and suspended upon a fine steel pivot over a graduated card placed a little before the centre of the instrument.

4. On account of the large relative size of the rectangular conductor of my galvanometer, the tangents of the deviations of the needle are very nearly proportional to the quantities of current electricity. The small correction which it is necessary to apply to the tangents, I obtained by means of the rigorous experimental process which I have some time ago described in the "*Annals of Electricity*."†

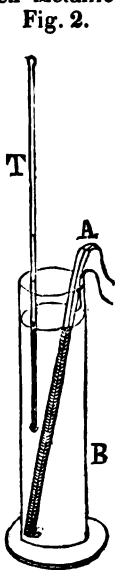
* Communicated by the author.

† Vol. iv, pp. 131, 132, and 476.

5. I have expressed my quantities of electricity on the basis of Faraday's great discovery of definite electrolysis, and I venture to suggest, that that quantity of current electricity which is able to electrolyze a chemical equivalent expressed in grains, in one hour of time, be called a *degree*. Now, by a number of experiments, I found that the needle of my galvanometer deviated $33^{\circ}5$ of the graduated card, when a current was passing in sufficient quantity to decompose nine grains of water per hour; that deviation, therefore, indicates one *degree of current electricity* on the scale that I propose to be adopted. We shall see in the sequel some of the practical advantages which I have had by using this measure.

6. The thermometer which I used had its scale graduated on the glass stem. The divisions were wide, and accurate. In taking temperatures with it, I stir the liquid gently with a feather, and then, suspending the thermometer, by the top of its stem, so as to cause it to assume a vertical position, I bring my eye to a level with the top of the mercury. In this way a little practice has enabled me to estimate temperatures to the tenth part of Fahrenheit's degree with certainty.

7. In order to ascertain the heating power of a given metallic wire, it was passed through a thin glass tube, and then closely coiled upon it. The extremities of the coil thus formed were then drawn asunder, so as to leave a small space between each convolution, and if this could not be well done, a piece of cotton thread was interposed. The apparatus thus prepared, when placed in a glass jar containing a given quantity of water, was ready for experiment. Fig. 2 will explain the dispositions: A is the coil of wire; B the glass jar partly filled with water; T represents the thermometer. When the voltaic electricity is transmitted through the wire, no appreciable quantity passes from it to take the shorter course through the water. No trace of such a current could be detected, either by the evolution of hydrogen, or the oxidation of metal.



8. Previous to each of the experiments, the necessary precaution was taken of bringing the water in the glass jar, and the air of the room to the same temperature. When this is accurately done, the result of the experiments bear the same proportions to one another as if no extraneous cooling agents, such as radiation, were present; for their effects in a given time are proportional to the difference of the temperatures of the cooling and cooled bodies; and hence, although towards the conclusion of some experiments this cooling effect is very considerable, the *absolute quantities* alone of heat are affected, not the *proportions* that are generated in the same time. [See the table of heats produced during half an hour and one hour, p. 290.]

9. Exp. 1.—I took two copper wires, each two yards long, one of

them $\frac{1}{8}$ th of an inch, the other $\frac{1}{32}$ th of an inch thick, and arranged them in coils in the manner that I have described (7). These were immersed in two glass jars, each of which contained nine ounces avoirdupois of water. A current of the mean quantity $1^{\circ}1$ Q,* was then passed consecutively through both coils, and at the close of one hour I observed that the water in which the thin wire was immersed had gained $3^{\circ}4$, whilst the thick wire had produced only $1^{\circ}3$.

10. Now by direct experiment, I found that three feet of the thin wire could conduct exactly as well as eight feet of the thick wire; and hence it is evident that the resistances of two yards of each were in the ratio of $3\cdot4$ to $1\cdot27$, which approximates very closely to the ratio of the heating effects exhibited by the experiment.

11. Exp. 2.—I now substituted a piece of iron wire $\frac{1}{8}$ th of an inch thick, and two yards long, for the thick copper wire used in Exp. 1, and placed each coil in half a pound of water. A current of $1^{\circ}25$ Q was passed through both during one hour, when the augmentation of temperature caused by the iron was 6° , whilst that produced by the copper wire was $5^{\circ}5$. In this case the resistances of the iron and copper wires was found to be in the ratio of 6 to $5\cdot51$.

12. Exp. 3.—A coil of copper wire were then compared with one of mercury, which was accomplished by inclosing the latter in a bent glass tube. In this way I had immersed each in half a pound of water, $11\frac{1}{4}$ feet of copper wire $\frac{1}{8}$ of an inch thick, and $22\frac{3}{4}$ inches of mercury $0\cdot065$ of an inch in diameter. At the close of one hour, during which the same current of electricity was passed through both the former had caused a rise of temperature of $4^{\circ}4$, the latter of $2^{\circ}9$. The resistances were found by a careful experiment to be in the ratio of $4\cdot4$ to 3 .

13. Other trials were made with results of precisely the same character; they all conspire to confirm the fact, that *when a given quantity of voltaic electricity is passed through a metallic conductor for a given length of time, the quantity of heat evolved by it is always proportional to the resistance which it presents,† whatever may be the length, thickness, shape, or kind of that metallic conductor.*

14. On considering the above law, I thought that the effect produced by the increase of the intensity of the electric current would be as the square of that element; for it is evident that in that case the resistance would be augmented in a double ratio, arising from the increase of the *quantity* of electricity passed in a given time, and also from the increase of the *velocity* of the same. We shall immediately see that this view is actually sustained by experiment.

15. I took the coil of copper wire used in Exp. 3. and have found the different quantities of heat gained by half a pound of water in

* I place Q at the end of my *degrees*, to distinguish them from those of the graduated card.

† Mr. Harris, and others, have proved this law very satisfactorily, using common electricity.

which it was immersed, by the passage of electricities of different degrees of tension. My results are arranged in the following table:—

Mean Deviations of the Needle of the Galvanometer.	Quantities of Cur- rent Electricity expressed in De- grees (5).	Quantities of Heat produced in half an hour by the Intensities in Co- lumn 2.	Ratio of the In- Squares of the In- tensities in Co- lumn 2.	Quantities of Heat produced in one hour by the Inten- sities in Column 2.	Ratio of the Squares of the Intensities in Column 2.
°	°	°		°	
16	0.33 Q			1.2	1
31½	0.92 Q	3	2.9	4.7	4.55
55	2.35 Q	19.4	18.8		
57¾	2.61 Q	23	23.2		
58½	2.73 Q	25	25.4	39.6	40.

16. The differences between the numbers in columns three and five, and those in columns four and six, are very inconsiderable, taking into account the nature of the experiments, and are principally owing to the difficulty which exists in keeping the air of the room in the same state of quiet, of hygrometry, &c., during the different days on which the experiments were made. They are much less when a larger quantity of water is used, so as to reduce the cooling effects. —(28).

17. We see, therefore, *when a current of voltaic electricity is propagated along a metallic conductor, the heat evolved in a given time is proportional to the resistance of the conductor multiplied by the square* of the electric intensity.*

18. The above law is of great importance. It teaches us the right use of those instruments which are intended to measure electric currents by the quantities of heat which they evolve. If such instruments be employed, (though in their present state they are far inferior in point of accuracy to many other forms of the galvanometer), it is obvious that the *square roots* of their indications are alone proportional to the intensities which they are intended to measure.

19. By another important application of the law, we are now enabled to compare the frictional† and voltaic electricities, in such a

* The experiments of De la Rive show that the calorific effect of the voltaic current increases in a much greater proportion than the simple ratio of the intensities. *Ann. de Chimie*, 1836, part i, p. 193. See also Peltier's results, *Ann. de Chimie*, 1836, part ii, p. 249.

† The experiments of Brooke, Cuthbertson and others, prove that the quantity of wire melted by common electricity is as the square of that battery's charge. Harris, however, arrived at the conclusion, that the heating power of electricity is *simply* as the charge, *Phil. Trans.*, 1834, p. 225. Of course the remark in the text is made on the presumption, that when the proper limitations are observed, the calorific effect of electricity is as the square of the charge of any given battery.

manner as to determine their elements by the quantity of heat which they evolve in passing along a given conductor; for if a certain quantity of voltaic electricity produce a certain degree of heat by passing along a given conductor, and if the same quantity of heat be generated by the discharge of a certain electrical battery along the same conductor, the product of the quantity and velocity of transfer of the *voltaic* electricity, will be equal to the product of the quantity and velocity of the *frictional* electricity, or $Q V = q v$, whence $\frac{Q}{q} = \frac{V}{v}$.

CHAP. II.—Heat evolved during Electrolysis.

20. Under the above head, I shall now examine the heat produced in the cells of the battery, and when electrolytes are experiencing the action of the voltaic current. It has been my desire to render these experiments strictly comparable, both with themselves and with those of other philosophers. I have therefore taken care to apply the corrections which either specific heat, or other disturbing causes might require, and have by these means been able to express in every case, the *total* amount of evolved heat.

21. The first of these corrections, which I call Cor. A, arises from the difference between the mean temperature of the liquid used in an experiment, and that of the surrounding atmosphere. Its amount is determined by ascertaining the rapidity with which the temperature of the liquid is reduced at the end of each experiment.

22. The second correction (Cor. B) is for the specific heat of the liquids, and the vessels which contain them; and when the necessary data could not be found in the tables of specific heat, I have supplied them from my own experiments. The *vessels* were white earthenware jars, $4\frac{1}{2}$ inches deep, and $4\frac{1}{4}$ inches in diameter: their caloric was one-twelfth of that contained by two pounds of water, to which capacity I have reduced all my subsequent results.

23. As resistance to conduction is the sole cause of the heat produced in the connecting wire of the voltaic battery, it was natural to expect that it would act an important part in this second class of phenomena also. It was important, therefore, to begin by determining the amount of heat evolved by that quantity of conducting metal which I found it convenient to adopt as a *standard of resistance*.

24. Ten feet of copper wire, 0.024 of an inch thick, were formed into a coil in the manner described in (7); its resistance to conduction was called *unity*. Three experiments were made in order to ascertain its heating power.

25. 1st. A jar was filled with two pounds of water, and a current which produced a mean deviation of the needle of the galvanometer (3) equal to $57\frac{1}{4}^{\circ} = 2^{\circ}.54$ Q of current electricity, was urged through the coil for twenty-seven minutes, by means of a zinc-iron*

* Whenever an iron battery was used, it was of course placed at a distance from the galvanometer sufficiently great to render its action on the needle altogether inappreciable.

battery of ten pairs. The heat thus acquired by the water, after Cor. A, and that part of Cor. B which relates to the caloric of the jar, had been applied, was $6^{\circ}22$.

26. 2nd. The battery was now charged with a weaker solution of sulphuric acid. In this case it passed the mean current $2^{\circ}085$ Q during forty-five minutes. The heat thus produced, when corrected, was $7^{\circ}04$.

27. 3rd. A battery of five pairs (three of which had platinized silver; one silver, and one copper, for their negative plates,) passed the mean current $1^{\circ}88$ Q during one hour, in which time $7^{\circ}47$ were generated.

28. When the first two experiments are reduced, in order to compare them with the third, we have, in accordance with the principles laid down in (17), $\frac{(1.88)^2}{(2.64)^2} \times \frac{60'}{27'} \times 6^{\circ}22 = 7^{\circ}57$,

and $\frac{(1.88)^2}{(2.085)^2} \times \frac{60'}{45'} \times 7^{\circ}04 = 7^{\circ}63$. Thus we have $\frac{7^{\circ}57 + 7^{\circ}63 + 7^{\circ}37}{3} = 7^{\circ}56$, the mean and total quantity of

heat produced per hour by the passage of $1^{\circ}88$ Q of current electricity, against the *unit* of resistance.

29. Before I proceed to give an account of some experiments on heat evolved in the cells of voltaic pairs, it is important to observe that every kind of action not essentially electrolytic must be eliminated. For instance, the dissolution of metallic oxides in acid menstrua, which has been proved by Dr. Faraday to be no cause of the current, is the occasion of a very considerable quantity of heat, which, if not accounted for in the experiments, would altogether disturb the results. I have taken the oxide of zinc, prepared either by igniting the nitrate, or by burning the metal, and have repeatedly dissolved it in sulphuric acid of various specific gravities; and on taking the mean of many experiments, none of which differed materially from the rest, I have found that the total corrected heat produced by the dissolution of 100 grains of the oxide of zinc in sulphuric acid, is able to raise two pounds of water $3^{\circ}44$.

30. Exp. 1.—I constructed a single voltaic pair, consisting of thin plates of amalgamated zinc and platinized silver (Mr. Smee's arrangement): the plates were two inches broad, and were kept one inch asunder by means of a piece of wood, to the opposite sides of which they were bound with string: to the top of each plate, a thick copper wire formed a good metallic connexion, by means of a brass clamp. The voltaic pair, thus prepared, was immersed in two pounds of sulphuric acid, sp. gr. 1.137, contained by one of the earthenware jars (22). The arrangement is represented by Fig. 3.

31. When the circuit was completed so as to present to the current the total metallic resistance 0.06, the galvanometer stood at $49\frac{1}{2}^\circ = 1.84 \text{ Q}$; and at $17\frac{1}{2}^\circ = 0.453 \text{ Q}$, when the total metallic resistance was increased to 1.16 by the addition to the circuit of ten feet of thin copper wire. Hence, according to the principles laid down by Ohm, $\frac{1.84}{r+1.16} = \frac{0.453}{r+0.06}$;

from which r , the resistance of the voltaic pair, = 0.299. Immediately after this trial, the temperature of the liquid being exactly 49° , and that of the air 50.2° , the circuit was completed for one hour, during which the needle first advanced a little from 50° , and then declined to 46° , the average* deviation was $48^\circ 44' = 1.8 \text{ Q}$. The temperature of the liquid was then 53.7° , indicating a rise of 4.7° .

Another trial now gave $\frac{1.59}{r'+1.16} = \frac{0.382}{r'+0.06}$; whence r' , the resistance of the pair at the close of the experiment, = 0.288: the mean resistance of the pair was therefore 0.293.

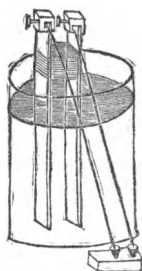
32. Now in order to obtain the total amount of heat evolved by the pair, reduced to the capacity of two pounds of water, we have $4.7 + 0.4$ (on account of Cor. A (21)) and -0.5 (on account of Cor. B (22)) = 4.6 . The correction due to the dissolution of oxide of zinc is found by multiplying its quantity by $\frac{3.44}{100}$ (see (29)); the

quantity of the oxide being obtained by multiplying the equivalent of oxide of zinc by the mean quantity of current electricity. We have then $4.03 \times 1.8 \times \frac{3.44}{100} = 2.5$: this, when subtracted from 4.6 , leaves 2.1 , the *correct voltaic heat*.

33. Assuming in this case, as well as in that of a metallic conductor, that the heat evolved is proportional to the resistance multiplied by the square of the electric intensity, we have, from the data in (28) and (31), $\frac{(1.8)^2}{(1.88)^2} \times 0.293 \times 7.56 = 2.03$, which is very near 2.1 , the heat deduced from experiment.

34. Exp. 2.—I now constructed another pair, consisting of plates precisely similar to those used in Exp. 1, but half an inch only asunder: it was also immersed in two pounds of sulphuric acid, sp. gr. 1.137. The circuit was closed for one hour, during which the needle of the galvanometer advanced gradually from $47\frac{1}{2}^\circ$, to $50\frac{1}{2}^\circ$, the mean deviation being $49^\circ 35' = 1.84 \text{ Q}$. The liquid had then gained 4.8° : this, $+0.1$ (for Cor. A) and -0.5 (for Cor. B), =

Fig. 3.



* During each experiment the deflections of the needle were noted at intervals of five minutes, or less. From thence I deduce my averages.

4°4. The heat due to the dissolution of oxide of zinc is in this case $4.03 \times 1.84 \times \frac{3^{\circ}44}{100} = 2^{\circ}55$, which, when subtracted from 4°4, leaves the correct voltaic heat 1°85.

35. The resistance of the pair was ascertained in this, as in every other instance, at the beginning and at the end of the experiment. The equations thus obtained were $\frac{1.714}{r + 1.16} = \frac{0.432}{r + 0.06}$, and $\frac{1.91}{r' + 1.16} = \frac{0.446}{r' + 0.06}$; whence $r = 0.311$, and $r' = 0.275$: the mean resistance was therefore 0.293. Now, calculating as before (33), on the basis of the heat produced by the passage of electricity against the standard of resistance, we have $\frac{(1.84)^2}{(1.88)^2} \times 0.293 \times 7^{\circ}56 = 2^{\circ}12$.

36. Exp. 3.—I formed another pair on Mr. Smee's plan; it was similar to the last, with the exception that the plates were only one inch broad. When the circuit was closed, a current of the mean intensity 1°46 Q passed through the apparatus during one half hour. The heat thereby produced, when corrected, and reduced on account of the dissolution of oxide of zinc, was 0.84.

37. In this instance the mean resistance was 0.32; whence, by a calculation precisely similar to those given under Exps. 1 and 2, we have the theoretical amount of heat = 0°74.

38. The three instances above given, are specimens taken from a number of experiments with the platinized silver pairs. The mean of the eight unexceptionable experiments which I have made with them, gives 2°08 of actual, and 2°13 of theoretical heat, and not one of the individual experiments presented a greater difference between real and calculated heat, than Exp. 2.

39. Exp. 4.—A plate of copper, four inches broad, was bent about a plate of amalgamated zinc three inches and a half broad, so as to form a pair of Wollaston's double battery. It was placed in a jar containing two pounds of dilute sulphuric acid. In this instance, the total voltaic heat that was generated was 1°2, the calculated result being 1°0 only. Repeated experiments with the copper pairs gave similar results, the real heat being invariably somewhat superior to that which the doctrine of resistances would demand. The cause of this I have found in a slight local action, which it is almost impossible to avoid in the common copper battery.

40. Exp. 5.—I now constructed a single pair on Mr. Grove's plan. The platinum, two inches broad, was immersed in an ounce and a half of strong nitric acid contained by a 4-inch pipe-clay cell; the amalgamated zinc plate, also two inches broad, was immersed (at the distance of an inch and a half from the platinum) in thirty ounces of sulphuric acid, sp. gr. 1.156. The whole was contained by one of the jars (22).

41. A trial, made first as usual, in order to ascertain the resistance

of the pair, gave $\frac{4.4}{r + 2.26} = \frac{0.816}{r + 9.06}$; whence $r = 0.441$.

As soon as the slight heat acquired during the above trial was equably diffused through the apparatus, the thermometer placed in the dilute sulphuric acid stood at $51^{\circ}.95$, the temperature of the air being $52^{\circ}.4$. The circuit was then immediately closed for ten minutes, during which time the needle of the galvanometer advanced steadily from $68^{\circ} 40'$ to $71^{\circ} 20'$; the mean deviation being $70^{\circ} 9' = 4^{\circ}.77$ Q. As soon as the heat thus generated was equably diffused*, the thermometer immersed in the dilute sulphuric acid stood at $56^{\circ}.7$, indicating a rise of $4^{\circ}.75$. Another trial now gave $\frac{5.14}{r' + 2.26} = \frac{0.91}{r' + 0.06}$, whence $r' = 0.413$. The mean resistance of the pair was therefore 0.427 .

42. $4^{\circ}.75 + 0^{\circ}.1$ (for Cor. A), and $- 0^{\circ}.4$ (for Cor. B, which in this case includes the capacity for heat of the porous cell) $= 4^{\circ}.45$. The heat generated by the dissolution of oxide of zinc was in this case $40.3 \times 4.77 \times \frac{3^{\circ}.44}{100} \times \frac{10}{60} = 1^{\circ}.1$, which, subtracted from $4^{\circ}.45$, leaves the correct voltaic heat $3^{\circ}.35$.

43. The *theoretical* result is $\frac{(4.77)^2}{(1.88)^2} \times 0.427 \times 7^{\circ}.56 \times \frac{10'}{60} = 3^{\circ}.46$.

44. Exp. 6 was made with a pair in every respect similar to the last: the circuit, however, was completed by means of a thin copper wire, in order to reduce the intensity of the current. At the end of one hour, during which the needle of the galvanometer advanced gradually from 41° to 42° , the correct voltaic heat that was generated was $1^{\circ}.7$. The *theoretical* result was $1^{\circ}.82$.

45. I was desirous of knowing how far the same principles would apply to the heat generated in Professor Daniell's constant battery. But in this battery considerable *cold* is produced, in consequence of separation of oxide of copper from the sulphuric acid to which it is combined. This is altogether a secondary effect, and should be eliminated as decidedly as the *heat* produced by the dissolution of oxide of zinc. I have not yet been able to obtain accurate data for the correction thus needed, and shall therefore content myself with remarking, that my result with Mr. Daniell's arrangements are, as far as they go, quite consistent with the theory of resistances.

46. Experiments, such as I have related, were varied in many ways; and sometimes a number of pairs were arranged so as to form a battery. Still the results were similar, and established the fact, that *the heat which is generated in a given time in any pair, by true voltaic action, is proportional to the resistance to conduction of that pair, multiplied by the square of the intensity of the current.*

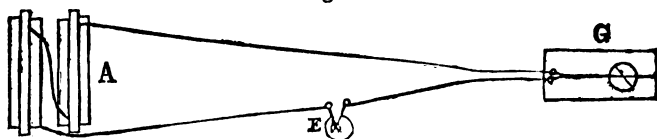
* By gently stirring the dilute sulphuric acid with a feather, so as to bring every part in successive contact with the porous cell during two minutes.

47. I now made some experiments on the heat consequent to the passage of voltaic electricity through electrolytes.

48. Exp. 7.—Two pieces of platinum foil, each of which was an inch long, and a quarter of an inch broad, were hermetically sealed into the ends of two pieces of glass tubing: within these tubes, pieces of copper wire were metallically connected with the platinum; these, when the apparatus was in action, terminated in mercury cups. The tubes thus prepared were bound together by thread, so as to keep the pieces of platinum foil at the constant distance of half an inch asunder. This apparatus was immersed in two pounds of dilute sulphuric acid, sp. gr. 1.164, contained in one of the jars (22).

49. A battery of twenty (four inch) double iron zinc plates, was then placed, with its divided troughs (which were charged with a pretty strong solution of sulphuric acid), at a distance from the galvanometer sufficiently great to obviate any disturbing effect on the needle. To the electrodes of this battery thick copper wires were secured, so that by means of one of them connexion could be made to the galvanometer, and by means of the other, to the decomposing cell. In Fig. 4, A represents the battery, G the galvanometer, and

Fig. 4.



E the decomposing apparatus (48).

50. In order to ascertain the resistances of the battery or of the cell, I provided several coils* of silked copper wire, the resistances of which had been determined by careful experiments. When these were traversed by the current, they were placed in such a position as to prevent any action on the needle, and at the same time they were kept under water, in order to prevent them from becoming hot, which would have had the effect of increasing their resistances.

51. When everything was duly prepared, the battery was placed in its troughs, and the current from it was urged through the galvanometer and each of the three of the coils, which were placed in succession at E (the decomposing apparatus having been removed). The resistances of these coils, were 4.4, 5.5, and 7.7, and the currents which they allowed to pass were 1.88 Q, 1.65 Q, and 1.29 Q.

52. The decomposing apparatus was now replaced, and the proper connexions being made, electrolytic decomposition was allowed to proceed during twenty minutes, in which time the needle of the galvanometer gradually declined from 55° to $48\frac{1}{2}^\circ$, the mean current

* Two of these coils had been previously employed (31, 41), &c., in ascertaining the resistances of the voltaic pairs: the resistance 0.06 was that of the galvanometer and connecting wires.

being $1^{\circ}9$ Q. The temperature of the liquid had now advanced from $46^{\circ}6$ to $53^{\circ}95$, indicating an increase of $7^{\circ}35$. The temperature of the surrounding atmosphere was $46^{\circ}4$.

53. The decomposing cell was now removed again, and the several coils, of which the resistances were, as before, $4\cdot4$, $5\cdot5$, and $7\cdot7$, were successively put in its place. The battery now urged through them, $1^{\circ}73$ Q, $1^{\circ}48$ Q, and $1^{\circ}22$ Q.

54. In this case $7^{\circ}35 + 0^{\circ}55$ (for Cor. A) and $- 0^{\circ}46$ (for Cor. B) = $7^{\circ}26$, the heat which was generated in the decomposing jar.

55. The mean intensity of the current when passing through the coil of which the resistance was $4\cdot4$, was $\frac{1^{\circ}88 + 1^{\circ}73}{2} = 1^{\circ}805$ Q, but $1^{\circ}9$ Q when it passed through the decomposing cell. Hence $(4\cdot4 + 3\cdot15) \frac{1^{\circ}805}{1\cdot9} = 7\cdot17$, this, $-3\cdot15^*$, leaves $4\cdot02$, the amount of *obstruction* presented by the decomposing cell.

56. Now we must remember, that when the electric current was passing through the coils, it was urged by the whole intensity of the battery; but that in the case of the decomposing cell, a part of the intensity of the zinc-iron battery, equal (as I have found by experiment) to $3\frac{1}{2}$ pairs, or to one-sixth part of the whole, is occupied solely in overcoming the *resistance to electrolyzation*† of water in the decomposing cell. In order therefore to deduce the true *resistance to conduction*, we must subtract $\frac{4\cdot02 + 3\cdot15}{6}$ from the *obstruction* $4\cdot02$; and thus we have $2\cdot83$, the true *resistance to conduction* of the decomposing cell.

57. The latter part of this process is difficult to express clearly, I have therefore drawn a figure to illustrate it. Suppose that in Fig. 5, 6 represents the intensity of the battery; the line R $3\cdot15$ the resistance of the battery and the connecting wires; and the remainder of the line A B, or $4\cdot02$ W, the resistance of wire. I have shown (55) that the current $1^{\circ}9$ Q would pass against the resistance

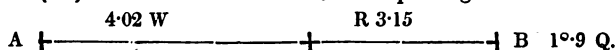
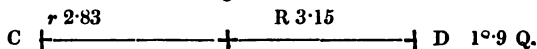


Fig. 5.



A B. But we know that $1^{\circ}9$ Q was also passed when the cell and the battery formed the sole opposition (52), and that on account of

* From (51 53,) we have the equations $\frac{1\cdot88}{R+7\cdot7} = \frac{1\cdot29}{R+4\cdot4}$ and $\frac{1\cdot73}{R'+7\cdot7} = \frac{1\cdot22}{R'+4\cdot4}$, whence $R = 2\cdot81$ and $R' = 3\cdot49$: the mean resistance of the battery and connecting wires was therefore $3\cdot15$.

† Faraday's Experimental Researches (1007).

the resistance to electrolyzation, the virtual battery intensity was then one-sixth less, and hence that only five-sixths of the resistance represented by A B could have been opposed in this case, in order to the passage of the same current. Draw, therefore, another line, C D, one-sixth less than A B, and it will represent this resistance; from which, on subtracting R 3·15, we have r 2·83, the true resistance to conduction of the decomposing apparatus.

58. From (28), and the data above given, we have $\frac{(1.9)^2}{(1.88)^2} \times$

$2.83 \times 7.56 \times \frac{20'}{60'} = 7.29$, the *theoretical* result.

59. I made three other experiments with the same electrodes, and with the same battery. The results of these with those of the experiment just given at length, are as follows:

	Experimental.		Theoretical.
Exp. 7.	7.26		7.29
Exp. 8.	8.12		8.32
Exp. 9.	10.2		10.2
Exp. 10.	9.64		9.75 (Refitted battery.)
Mean	8.8		8.89

60. Exp. 11.—The mean current 0.846 Q from a battery of ten zinc-iron pairs, was, by means of the same electrodes, sent through two pounds of dilute sulphuric acid for half an hour, during which the correct heat that was generated was 3.09 .

61. In order to find the true resistance to conduction of the decomposing cell, it was necessary to remember that in this instance one-third of the intensity of the ten pairs was expended in overcoming the resistance to electrolyzation of the water. With this exception the calculations were made precisely as before, and gave 3.76 , the resistance of the cell; whence we have the theoretical heat 2.88 .

62. I now dismissed the narrow electrodes, and substituted for them two pieces of platinum foil, dipping to the bottom of the liquid; they were one inch apart, and each presented to the dilute sulphuric acid a surface of seven square inches. In this case I used twenty pairs of zinc-iron plates arranged in a series of ten.

63. The mean of six experiments with this apparatus gave 4.42 of real, and 4.13 of theoretical heat. I have no doubt that the difference is principally occasioned by the formation of the deutoxide of hydrogen, which is known to occur to a considerable extent when oxygen is evolved from an extended surface. Of this we have another instance in the following experiment.

64. Exp. 12.—Using the same electrodes, and a battery of ten zinc-iron pairs, I now passed a current of the mean intensity 1.08 Q, through two pounds of dilute nitric acid, sp. gr. 1047, for half an hour. The heat that was thus generated, when properly corrected, was 3.0 .

65. This experiment was, as the others, conducted in the manner described at length under Exp. 7. Water chiefly* was decomposed; and I ascertained, experimentally, that about $\frac{1}{3.5}$ of the intensity of the battery was expended in overcoming resistance to electrolysis. Thus I had $3.52 - \frac{3.52 + 1.68}{3.5} = 2.03$, the resistance to conduction; and hence $\frac{(1.08)^2}{(1.88)^2} \times 2.03 \times 7^{\circ}.56 \times \frac{30'}{60'} = 2^{\circ}.53$, the theoretical heat.

66. Exp. 13.—Two plates of copper, each of which was two inches broad, were secured at the distance of one inch apart, and immersed in two pounds of a saturated solution of sulphate of copper. Through this apparatus, a battery of ten zinc-iron pairs passed the mean current 1° Q, during half an hour. The heat thus produced, when properly corrected, was $5^{\circ}.8$.

67. In this case there was no *resistance of electrolysis*, and the action may be regarded simply as a transfer of copper from the positive to the negative electrode. All the *obstruction*, therefore, that was presented to the current, was *resistance to conduction*.

Its mean was 5.5 , whence we have $\frac{1}{(1.88)^2} \times 5.5 + 7^{\circ}.56 + \frac{30'}{60'} = 5^{\circ}.88$, the theoretical heat.

68. We have thus arrived at the general conclusion, that *the heat which is evolved by the proper action of any voltaic current is proportional to the square of the intensity of that current, multiplied by the resistance to conduction which it experiences*. From this law the following conclusions are directly deduced:—

69. 1st. That *if the electrodes of a galvanic pair of given intensity be connected by any simply conducting body, the total voltaic heat generated by the entire circuit (provided always that no local action occurs in the pair) will, whatever may be the resistance to conduction, be proportional to the number of atoms (whether of water or of zinc) concerned in generating the current*. For if the resistance to conduction be diminished, the quantity of current will be increased in the same ratio, and hence, according to the law (68), the quantity of heat which would thus be generated in a given time will be also proportionally increased; whilst of course the number of atoms which would be electrolyzed in the pair will be increased in the same proportion.

70. 2nd. That *the total voltaic heat which is produced by any pair, is directly proportional to its intensity, and the number of atoms which are electrolyzed in it*. For the quantity of current is proportional to the intensity of the pair, and consequently the quantity of heat evolved in a given time is proportional to the square of the

* See Faraday on the electrolysis of nitric acid, "Experimental Researches" (752).

intensity of the pair, but the number of atoms electrolyzed is proportional, in the same time, to the simple ratio only of the current, or of the intensity of the pair.

71. And 3rd. That *when any voltaic arrangement, whether simple or compound, passes a current of electricity through any substance, whether an electrolyte or not, the total voltaic heat which is generated in any time, is proportional to the number of atoms which are electrolyzed in each cell of the circuit, multiplied by the virtual* intensity of the battery.*

72. Berzelius thinks that the light and heat produced by combustion are occasioned by the discharge of electricity between the combustible and the oxygen with which it is in the act of combination; and I am of opinion that the heat arising from this, and some other chemical processes, is the consequence of resistance to electric conduction. My experiments on the heat produced by the combustion of zinc turnings in oxygen (which, when sufficiently complete, I shall make public) strongly confirm this view; and the quantity of heat which Crawford produced by exploding a mixture of hydrogen and oxygen may be considered almost decisive of the question. In his unexceptionable experiments, one grain of hydrogen produced heat sufficient to raise one pound of water $9^{\circ}6$. Now we know from Exp. 5, that the heat generated in one of Mr. Grove's pairs by the electrolysis of $\frac{4.77 \times 32.3}{6} = 25.7$ grains of zinc, is theoretically $3^{\circ}46$; and the heat which must in the same time have been generated by the *metallic* part of the circuit, which presented the resistance 0.06 , is $\frac{0.06}{0.427} \times 3^{\circ}46 = 0^{\circ}48$; the total voltaic heat was therefore $3^{\circ}94$. Hence the total heat which would have been evolved by the electrolysis of an equivalent, or 32.3 grains of zinc, is $\frac{32.3}{25.7} \times 3^{\circ}94 = 4^{\circ}95$; which, when reduced to the capacity of one pound of water, is $9^{\circ}9$. But from the table of the intensities of voltaic arrangements (74), the intensity of Mr. Grove's pair, compared with the affinity of hydrogen for oxygen, is $\frac{1}{0.93}$ whence, from (70), we have $9^{\circ}9 \times 0.93 = 9^{\circ}2$, the heat which should be generated by the combustion of one grain of hydrogen, according to the doctrine of resistances; the result of Crawford is only $0^{\circ}4$.

73. I am aware that there are some anomalous conditions of the current which seem to militate against the general law (68), particularly when in the hands of Peltier it actually produces cold.†

* If a decomposing cell be in the circuit, the *virtual* intensity of the battery is reduced in proportion to its resistance to electrolyzation.

† If antimony and bismuth be soldered together, cold will be produced at the point of junction by the passage of the current from the bismuth to the antimony. Peltier, *Annales de Chimie*, vol. lvi, p. 371. In his paper, however, a misprint has inverted the direction of the current.

I have little doubt, however, that the explanations of these will be ultimately found in actions of a secondary character.

Note on Voltaic Batteries.

74. In the foregoing investigation I have had occasion to work very extensively with different voltaic arrangements, and have repeatedly ascertained their relative intensities by the mathematical theory of Ohm. It will not, therefore, I hope, be deemed out of place to subjoin a table, in which the intensities of the batteries which are most generally used, are inversely as the number of pairs which would be just requisite in order to overcome the resistance of water to electrolyzation.

Mr. Grove's	{ Platinum Nitric acid	{ Amalgam. zinc Dilute sulph. acid	{ $\frac{1}{0.93}$	} Constant Intensities.
Prof. Daniell's	{ Copper Sulphate of oxide of copper	{ Amalgam. zinc Dilute sulphuric acid	{ $\frac{1}{1.54}$	
Mr. Sturgeon's	{ Iron. Amalgamated zinc Dilute sulphuric acid		{ $\frac{1}{3.33}$	
Mr. Smee's	{ Platinized silver.	Amalg. zinc	{ $\frac{1}{3.58}$	
	{ Dilute sulphuric acid Copper. Algamated zinc Dilute sulphuric acid		{ $\frac{1}{5.40}$	

75. Without entering particularly into the respective merits of these arrangements, I may observe that each of the first four may be used advantageously, according to the circumstances in which the experimenter is placed, or the particular experiments which he wishes to execute. The zinc-iron battery is somewhat inconvenient on account of local action on the iron; but then it presents great mechanical facilities in its construction. Mr. Smee's and Mr. Grove's are also very good arrangements; but the battery of Daniell is the best instrument for general use, and is, moreover, unquestionably the most economical.

Broom Hill, Pendlebury near Manchester,
March 25th, 1841.

P.S. In the above table of galvanic intensities, that of zinc-iron immersed in dilute sulphuric acid is somewhat overstated. Recent experiments convince me that when the iron is in its best condition it possesses the same powers as the platinized silver. I attributed the iron battery to Mr. Sturgeon, who constructed one of these excellent instruments early in 1839. It consisted of twelve cast-iron tubes, furnished with strips of amalgamated zinc. But I find that the experiments of this gentleman were not published as early as those of Mr. Roberts. Prof. Daniell (Phil. Trans. 1836, p. 114) observed that iron is sometimes more efficient than platinum in voltaic association with amalgamated zinc.

J. P. J.

August 11, 1841.

On the Electric Origin of the Heat of Combustion.

By J. P. JOULE, Esq.*

[Illustrated by Plate II.]

1. In the papers which I had some time ago the honour of communicating to the Royal Society. I related an investigation concerning the calorific effects of voltaic electricity, and stated my opinion with regard to the heat evolved by combustion and certain other chemical phenomena. In the present paper I intend to bring forward some experiments in confirmation of my theory, and to prove that the heat of combustion, terminating in the formation of an electrolyte, is the consequence of resistance to electric conduction.

2. We have seen that when those chemical actions which are not the sources of transmitted electricity are allowed for, the heat evolved from any part of the voltaic apparatus is the effect of the resistance which is presented by that part to the electric current; and that hence it necessarily follows, that the total voltaic heat generated by the action of any closed galvanic pair is proportional to the number of chemical equivalents which have been consumed in the act of propelling the current, and the intensity of the galvanic arrangement. Now, if it can be shown that the quantity of heat which is evolved by ordinary chemical combination is the same as the calculation founded on these facts would lead us to expect, no reasonable doubt can be entertained that it also is the product of resistance to electric conduction.

3. In studying the character of the heat of combustion, the first point was to determine the intensities of the affinities of different combustibles for oxygen. For this purpose I have, in accordance with the views which were first stated by Davy, and have since been adopted by the most eminent electricians, made use of the measure of those intensities which is afforded by the electric current.

4. I had not proceeded far before some curious phenomena were observed, which, though not very well understood, have long been known† to electricians. I shall notice these first, because of their important bearing upon subsequent reasonings and conclusions.

5. I was working with an arrangement consisting of iron, platinized silver, and dilute sulphuric acid. The circuit was closed by a galvanometer, the coil of which consisted of 119 turns of thin silked copper wire, forming a rectangle, measuring one foot by six inches. The needle indicated a pretty constant deviation of 20° , but on moving the platinized silver backwards and forwards the

* Read before the Literary and Philosophical Society of Manchester, November 2, 1841; and now communicated by the author.

† In 1830, Mr. Sturgeon remarked that when two pieces of iron are placed in dilute muriatic acid, the *agitation* of one of them will make it operate as copper in the copper-zinc battery: also, that if two pieces of iron are immersed in succession in a solution of nitrous acid, the iron *last* immersed will act as copper in the copper-zinc battery.—Recent Experimental Researches, p. 46—49. We shall hereafter see the true cause of these phenomena.

needle advanced gradually to 40° , where it was kept for some time by continuing the agitation. As soon as the motion of the platinized silver was discontinued, the needle resumed its former position. Similar effects were produced by stirring the liquid, and thus causing it to impinge against the platinized silver.

6. I repeated the above experiment many times with similar results, but I found that whenever a large quantity of hydrogen had been evolved from the liquid by the action of the pair, or otherwise, the phenomena were not well produced. This circumstance convinced me that the effects were due to atmospheric air held in solution by the liquid, and that the displacement of a part of it by the hydrogen had occasioned their partial prevention. My opinion was confirmed by the following experiment.

7. I filled three quarters of the contents of a glass flask with dilute sulphuric acid, and then placed it over the flame of a spirit-lamp until I judged that all the atmospheric air had been boiled out, I then removed the lamp, and immediately placed in the mouth of the flask a cork, through which a small piece of platinized silver and a stout iron wire had been passed. On connecting the metals with the galvanometer (5) its needle was deflected to $32\frac{1}{2}^{\circ}$, and on shaking the flask very briskly it could not be made to advance further than 34° . This advance, slight as it is, was probably entirely occasioned by the air, which, notwithstanding my precautions, had found its way into the upper part of the flask.

8. The phenomena originated entirely from the platinized silver; and although a slight advance of the needle was sometimes produced by agitating the *iron*, it was not difficult to see that the real cause was the propulsion thereby occasioned of the aerated liquid against the negative* element, for when this was avoided no advance of the needle could be produced by agitating the positive metal.

9. I thought it probable that an increase of the intensity of the current would be produced by directing a stream of oxygen gas against the negative element. On making the experiment, I found that the needle advanced a few degrees, and that the same effect could be produced by a stream of hydrogen. There could be no doubt that the increase of intensity arose rather from the agitation of the liquid than from any specific action of the gases, and that this experiment was essentially the same as that described in (5).

10. I impregnated some dilute sulphuric acid with a very small quantity of oxygen, according to Thenard's process, and then immersed into it a plate of platinized silver and a rod of iron, both properly communicated with the galvanometer. The needle stood for the first few seconds at 68° ; in three minutes it declined to 50° ; in five minutes more to 49° , and in another five minutes to $48\frac{1}{2}^{\circ}$. On agitating the platinized silver so as to bring it repeatedly in con-

* To avoid misconception, it is perhaps as well to observe that I call those elements of the voltaic battery *negative*, which attract or combine with those bodies which are called "positively electrical," or "cations."

tact with the yet undecomposed deutoxide of hydrogen, the needle advanced to above 60° . The same pair, immersed in common acid, would have deflected the needle no further than 29° or 30° .

11. The effect of the presence of oxygen at the negative element is well observed by making it, in water, the positive electrode of a voltaic battery. By this means oxygen is deposited on its surface, and is there ready to produce an extraordinary intensity. This deposit of oxygen is in fact the cause of the action of Ritter's secondary piles.

12. The following was also a very convenient method of showing the increase of intensity arising from the presence of oxygen. Some dilute sulphuric acid was agitated with chlorine until the former had taken up as much of the gas as it could. By pouring a solution of sulphate of oxide of silver into the liquid, I now precipitated chloride of silver, leaving sulphuric acid and oxygen in solution. When a pair, consisting of platinized silver and iron, was placed in the acid thus prepared, the galvanometer was permanently deflected 50° , and by agitating the platinized silver the needle advanced as far as 60° . When a piece of amalgamated zinc was substituted for the iron the permanent deflection of the needle was 65° , and by agitating the negative element as before, the needle advanced to 70° . Had the same pairs been immersed in a simple solution of sulphuric acid, the permanent deflections would have been no greater than 30° and 63° .

13. Similar results were obtained with the solution of chlorine, as might have been anticipated from its strong affinity for hydrogen.

14. From the above experiments, we see that the agitation of the negative element is productive of an increase of intensity, simply because it is thereby brought into contact with bodies capable of combining with the hydrogen, which would otherwise have been evolved from it. When those bodies are present in considerable quantities, as in (10), (12), and (13), the intensity of the current is great, even though the pair be left quiet, because then the negative plate can collect them readily upon its surface. Again, by causing the current to encounter great resistance, the effects of agitation which we have noticed are proportionally increased, because then the number of particles required for neutralizing the hydrogen is less. Hence it is that when I have used a resistance of 500 or 600 yards of thin wire, I have frequently found the deviation of the needle (even when the pair was left quiet in a common solution of sulphuric acid) considerably greater than was due to that resistance. This is also the probable reason why De la Rive in one instance* found the intensity of the copper-zinc pile the same, whether charged with water or nitric acid.

15. In the course of the preceding experiments I was forcibly struck with the very great intensity of the pairs at the moment of

* *Ann. de Chimie*, 1836, part i, p. 179.

their immersion, compared with that which they were able to maintain permanently. It appeared to me that the theories which had been put forth to explain the first effect of immersion, though seemingly plausible with regard to the zinc battery, were not at all equal to account for the same phenomenon as existing to a far greater extent when iron is used as a positive element.

16. A rod of iron and a small plate of platinized silver were immersed in a dilute solution of sulphuric acid. On connecting them with the galvanometer (5), the needle was permanently deflected $29\frac{1}{2}^{\circ}$. After a few preliminary trials to ascertain the proper point, I caused the needle to be maintained by a glass weight at 55° , beyond which it was free to travel. I then exposed the platinized silver to the air during one minute of time. On re-immersing it the needle sprang as far as 60° , and then immediately recoiled to its resting place at 55° , thus indicating a *transitory* current of about $57\frac{1}{2}^{\circ}$.

17. On exposing the platinized silver for 5" only, the transitory current, ascertained in a manner similar to that just mentioned, was 41° .

18. Greater effects were obtained by washing and drying the platinized silver before it was immersed. In this way the needle, adjusted at 62° , would spring as far as 66° , indicating a transitory current of about 64° . Having now removed the glass weight, the needle took up a permanent position at $29\frac{1}{2}^{\circ}$, as at the beginning of the experiments.

19. When, instead of the platinized silver, the positive element (iron) was exposed to the air, whether simply or in conjunction with washing and drying, no appreciable increase of intensity was occasioned by its immersion. And although, on a repetition of the experiment, I sometimes observed slight effects, I conceive that they were owing to the power which the negative element seems to possess of collecting upon its surface the air held in solution by the circumambient liquid.

20. With an arrangement of platinized silver and amalgamated zinc, I obtained results of a similar though less striking character. The galvanometer indicated a permanent deflection of 62° , and after washing and drying the platinized silver, I had a transitory deviation of 72° . The immersion of the amalgamated zinc, after washing and drying, produced no effect.

21. The *maximum* effects of immersion were produced in the following manner. A plate of silver was rubbed with a little nitric acid, and then exposed to a red heat, by means of which the film of nitrate of oxide of silver was decomposed and metallic silver reduced*. When the plate prepared in this way was associated in

* By this process all the oxygen is not driven off, but a considerable quantity remains adhering to the silver so tenaciously that it is not entirely removed by making the plate quite bright with glass paper. The oxygen thus deposited (it can hardly, I think, be considered as chemically combined with the silver) is the cause of the great intensity of the current immediately after immersion. By simply heating the silver to redness the same general effects can be produced, though not to the same extent.

dilute sulphuric acid with a piece of iron, the needle would deviate $63\frac{1}{2}^{\circ}$ for some time, and then gradually decline until it took up a permanent situation at $29\frac{1}{2}^{\circ}$. By experimenting in the same way with amalgamated zinc as a positive element, I had a transitory deflection of 76° , and a permanent deflection of 63° .

22. Very trifling transitory effects were obtained by the immersion of iron, when that metal was associated with amalgamated zinc. But this might have been anticipated, because the transitory current is owing to the presence of oxygen on the negative plate; and it is obvious that the hydrogen evolved by the local action of the iron, would, whilst in a nascent state, combine with that oxygen, and thus prevent a great part of it from exercising any influence upon the intensity of the current.

23. An experiment was also made with an arrangement of copper, amalgamated zinc, and dilute sulphuric acid. It was able to deflect the needle 51° pretty permanently. On washing and drying the copper, and experimenting as in (16), I observed a transitory deflection of 72° . This experiment deserves attention, because it shows that the transitory current occasioned by the copper is the same as that exhibited by platinized silver when experimented with in the same way (20). I take it as an argument, that when copper is in its best state it forms with amalgamated zinc a battery as intense as the platinized silver.

24. That the transitory currents which we are discussing are not occasioned by the diffusion of the salt formed about the positive element during the cessation of voltaic action, is obvious from the fact that (when the proper precautions are observed) they are not produced by the agitation (8), or by the immersion (19 and 20) of the metal about which the salt is formed. And if anything can render this more evident, it is the fact that the immersion of the copper plate of a Daniell's battery causes the needle to advance little or no higher than its permanent situation, as might have been anticipated from the theory which refers the transitory effects to chemical combination at the negative plate, on account of the slight affinity of copper for oxygen. The following experiments are also decisive of this question.

25. A glass jar, *a*, Fig. 1, containing some dilute sulphuric acid, was placed upon the plate *pp* of an air-pump. A small rod of iron, *i*, was immersed in the liquid, and connected by means of the pump-plate to the galvanometer (5). An open receiver, *r*, was now placed over the jar, and the ground brass plate *b*, with its stuffing-box and sliding rod (the latter having the small piece of platinized silver, *s*, affixed to its extremity), was placed on the top of the receiver. A copper wire, fastened to the ring of the sliding rod, connected the platinized silver with the galvanometer.

26. The sliding rod was now moved until the platinized silver in connexion with it was immersed in the acidulated water. Then the pump was worked until a very excellent vacuum was obtained, and so tight was every part of the apparatus that it could be left alone for

half an hour without the admission of any appreciable quantity of air. The galvanometer indicated a permanent deflection* of 27° . I now placed a piece of glass so as to prevent the needle from going lower than 27° , and by means of the sliding rod I removed the platinized silver entirely out of the acid. After it had been exposed during a quarter of an hour, I re-immersed it, when the needle sprang from 27° to 30° and back, indicating a transitory deflection of about $28\frac{1}{2}^{\circ}$. Although the defect of immersion exhibited by this experiment is extremely small, it appeared to be almost entirely occasioned by the repose of the electric condition of the iron, for when, instead of entirely withdrawing the platinized silver, its extremity was just allowed to touch the liquid, the transitory deflection was only $27\frac{1}{2}^{\circ}$, after an exposure during a quarter of an hour.

27. On admitting a quantity of air into the receiver sufficient to counterbalance the pressure of one inch of mercury, the effects of immersion were considerable after a very short exposure of the platinized silver. In a quarter of an hour it collected upon its surface sufficient oxygen to cause the needle to spring from 27° to 78° , whether it had or had not remained in contact with the liquid during its exposure.

28. When, instead of the vacuum, I used an atmosphere of hydrogen, the exposure of the platinized silver for any length of time did not render the current more intense at the moment of immersion than it remained permanently. And even when the hydrogen was diluted with one quarter of its bulk of atmospheric air the transitory effects did not appear, on account, no doubt, of the union† of the oxygen with the hydrogen as fast as the former, or both, collected upon the plate. On using a mixture of equal bulks of hydrogen and air, the transitory effects were very small, even after the platinized silver had been exposed for ten minutes.

29. I made several experiments with carbonic acid, but the transitory currents did not entirely disappear as was anticipated. The gas, though prepared carefully and in different ways, could not be obtained perfectly pure, and when exposed to an alkaline solution, $\frac{1}{100}$ th of it would remain uncondensed. In order therefore to remove any free oxygen which the gas might contain, I exposed it during two days and two nights to the action of a stick of phosphorus. After this, immersion caused no, or at most, very trivial transitory effects; but on admitting only one per cent. of oxygen they became very considerable,—a striking example of the power possessed by metals of collecting and condensing oxygen upon their surfaces. I do not bring forward this experiment as a proof of the

* No change in the permanent deflection of the needle was occasioned by the removal of atmospheric pressure.

† The phenomenon of Döbereiner, so fully investigated by Faraday, to whose paper, published in the Phil. Trans. for 1834, I refer the reader for some valuable observations on the power possessed by metals of condensing gases upon their surfaces.

entire non-action of carbonic acid, because the phosphorous was found to have decomposed it partially.

30. All these phenomena are easily understood, if, with the great body of philosophers, we keep in view the intimate relation which subsists between chemical affinity and the electric current. For let p , Fig. 2, represent a plate of platinum; z , a plate of zinc, or other electro-positive metal; and e , one of a series of atoms of water extending from p to z . The intensity of the current along the wire w , is proportional to the affinity of oxygen for the positive metal, minus the affinity of oxygen for hydrogen. But if p be covered with a film of oxygen, the current will be entirely proportional to the affinity of the positive metal for oxygen. In the former case, $c = z - h$; in the latter, $c' = z$.

31. Considering these equations, it is obvious why, as I have observed (15), the transitory currents are better exhibited with iron than with zinc as a positive element; for in proportion to the smallness of z , provided it remain greater than h , will the difference between c and c' be more manifest. If $c' - c$ be the same for both iron and zinc, we shall have a proof of the accuracy of these principles,

32. Thus from (21), turning the deflections of the needle into quantities of electricity, we have $63\frac{1}{2}^\circ = 0^\circ.034$ Q, and $29\frac{1}{2}^\circ = 0^\circ.0072$ Q, of which the difference is $0^\circ.0268$ Q, when *iron* is the positive element. We have also $76^\circ = 0^\circ.056$ Q, and $63^\circ = 0^\circ.027$ Q, of which the difference is $0^\circ.029$ Q, when *zinc* is the positive element. I consider these differences as nearly equal as could have been expected from the nature of the experiments.

33. I might now proceed to consider in detail several phenomena (such as the very rapid corrosion of metals when they are exposed to the joint action of air and moisture, &c.) which are occasioned by the great intensity of galvanic action, in consequence of the mixture of oxygen with the liquid. But I hasten to fulfil my principal design.

Intensities of the Affinities which unite bodies with Oxygen.

34. In order to ascertain the intensities of galvanic arrangements we may either use a galvanometer furnished with a short and thick, wire, or with a long and thin wire (within certain limits (14)). In the former case, the calculations must be conducted on the principles of Ohm; in the latter it is only necessary to take care that the resistances of the pairs under comparison are pretty nearly equal, in order that the deviations of the needle may be depended upon in calculating the intensity of the current. I have adopted the latter plan on account of the superior facilities which it presents.

35. *Affinity of zinc for oxygen.*—From (32) we have the intensity of the action of zinc $= 0^\circ.056$ Q; and the intensity required for the electrolysis of water $= 0^\circ.020$ Q. Hence $29 : 56 :: 1$, the affinity of hydrogen, $: 1.93$, that of zinc for oxygen.

36. *Affinity of iron for oxygen*, likewise obtained from (32) is $1^\circ.27$, for $268 : 340 :: 1 : 1.27$.

37. *Affinity of potassium for oxygen.*—Twenty grains of potassium were combined with about ten ounces of mercury. The amalgam was poured into a wooden cup, into the bottom of which a copper wire connected with the galvanometer (5) had been let. At about half an inch above the surface of the amalgam I secured a piece of platinum, also in connexion with the galvanometer. On pouring dilute sulphuric acid into the cup the needle was deflected 74° ($= 0^\circ.05 Q$) during three successive minutes, but the local action of the amalgam was so vigorous that at the end of this interval of time most of the potassium was dissolved, and the needle declined very fast. On treating 20 grains of zinc in precisely the same manner, I had a deviation of 49° ($= 0^\circ.0152 Q$). Hence

$$k - h = 0.05,$$

and

$$z - h = 0.0152,$$

whence

$$152 k = 500 z - 348 h.$$

But from (35), $z = 1.93$, and $h = 1$; therefore k , the affinity of potassium, $= 4.06$.

38. It is necessary, however, to pay attention to the circumstances under which the experiments were made, in order to obtain correct ideas concerning the above intensities of affinity. The increase of the intensity of the voltaic apparatus by heat is by no means great; and as all the experiments were conducted at common temperatures, no regard need be paid to it. But then the intensities of affinity were obtained by comparing currents which had been produced under peculiar circumstances with regard to the *condition* of the elements of the galvanic arrangements; in one case the hydrogen was evolved in a *gaseous* state; whilst in the other, the hydrogen, by combining with free and condensed oxygen, did not escape. Now we shall see from the following experiments that electric intensity is *expended* in the act of converting a body into the gaseous state.

39. I took ten glass jars (see Fig. 3), made them perfectly clean and dry*, and placed them in series on a non-conducting substance. Into these I poured a quantity of dilute sulphuric acid, taking care not to wet the glass within an inch of the top of each. Pairs of platinized silver and amalgamated zinc were placed in the jars, and connexions, furnished with the mercury cups 1, 2, 3, &c., were established between them *seriatim*. A decomposing cell, *d*, furnished with platinum wires, was connected on one hand with the battery, and on the other with the galvanometer (5). Lastly, I provided a copper wire, *w*, by means of which connexion could be conveniently made between the galvanometer *g*, and any of the mercury cups, 1, 2, 3, &c.

40. Into *d* I poured a small quantity of dilute sulphuric acid. Then, by placing the wire *w* in each of the mercury cups, beginning

* It is necessary to be very careful in insulating the apparatus, in order to obtain the *maximum* intensity of a battery. The divided porcelain trough has frequently great conducting powers (particularly when the glaze has been partially destroyed), which render it unfit for accurate experiments.

at 10 and ending at 10, I observed the deviations of the galvanometer contained in the following table.

No. of pairs in action.	Deflections of Galvanometer.			Comparative quantities of Electricity.
	Down.	Up.	Mean.	
10	$72\frac{1}{2}$	71^0	$71^0 \quad 45'$	341
9	69	68	68 30	284
8	$65\frac{1}{4}$	65	65 7	236
7	60	60	60 0	188
6	53	$53\frac{2}{3}$	53 20	141
5	42	43	42 30	$94\frac{1}{2}$
4	$25\frac{1}{2}$	$27\frac{3}{4}$	26 37	$48\frac{1}{2}$
3	$5\frac{1}{3}$	$5\frac{1}{3}$	5 30	$8\frac{1}{3}$
2	0	0	0 0	0
1	0	0	0 0	0

41. Now if we divide the straight line, AB, Fig. 4, into ten equal parts, representing pairs on Mr. Smee's plan, and if at each division we erect straight lines, perpendicular to AB and proportional to the comparative quantities of electricity just given, the principles of electric action demand that the line drawn through the extremities of those perpendiculars should be straight. It is in fact so nearly a straight line, that its slight discrepancies therefrom may be properly referred to unavoidable errors of experiment. Produce the straight line CD so as to meet AB in X, and the straight line AX, equal to 2·8, will indicate the number of pairs necessary to decompose water.

42. Fig. 5 represents an experiment of the same kind, with a solution of sulphate of oxide of zinc in the decomposing cell. Oxide of zinc was decomposed, the oxygen being evolved at the positive, and the zinc being reduced at the negative electrode. The intensity necessary to decompose oxide of zinc is equal to that of 3·7 of Mr. Smee's pairs.

43. With sulphate of protoxide of iron I did not at first succeed, on account of the formation of peroxide at the positive electrode. However, by placing the negative electrode among some crystals of the salt, pouring water thereon, and suspending the positive electrode in the water, I obviated that difficulty, and obtained the results which are projected in Fig. 6, and which indicates 3·3 pairs as the intensity necessary to decompose protoxide of iron.

44. Now from (41) and (42) we have (using the same letters as before) 2·8 pairs = h , and 3·7 pairs = z ; whence $2·8 z = 3·7 h$, and $z = 1·32 h$; or, in other words, the intensity required to separate oxide of zinc into metal and gas is to the intensity required to separate water into its gaseous elements as 1·32 : 1. But from (35), the

intensity produced by the union of *non-gaseous* oxygen with zinc is to the intensity necessary to separate water into *non-gaseous* oxygen and *gaseous* hydrogen, as $1.93 : 1$; and $1.32 : 1 :: 1.93 + 1.9 : 1 + 1.9$. Wherefore, the intensity necessary to give oxygen the gaseous form is to the intensity necessary to separate water into non-gaseous oxygen and gaseous hydrogen as $1.91 : 1$.

45. Thus we see that a very great intensity of current is employed in changing the condition of bodies, as well as in separating them from their combinations. The field of investigation here opened is very extensive, but I may not at present enter further upon it. I will only remark, that if the intensity necessary to convert a body into a different state, compared with the heat or cold due to the mechanical or other production of that different state, be such as accords with the relations of intensity and heat which we observe in the voltaic apparatus, we have a proof that some of the effects which are usually referred to "latent heat," are in fact nothing more than the recondite operations of resistance to electric current.*

46. In our investigation into the cause of the heat of combustion, it will be necessary to deduce our calculations from the electric intensity which is required in order to reduce the product of combustion to the state in which its elements were prior to combustion. The following is a list of these intensities, reckoning the decomposition of water into its gaseous elements as unity.

47. *Intensity necessary to decompose oxide of zinc into gaseous oxygen and metal*, from (42) and (44), is 3.7 pairs of Smee's battery, or $1.32 h$.

48. *Intensity necessary to decompose protoxide of iron into gaseous oxygen and metal*.—From (43), 3.3 of Smee's pairs = i ; and from (41), 2.8 pairs = h ; whence $2.8 i = 3.3 h$, or $i = 1.18$.

49. *Intensity necessary to decompose potassa into potassium and gaseous oxygen*.—From (144) and (37) we have $1.93 + 1.9 : 4.06 + 1.9 :: 1.32 h : 2.05 h$ the intensity required; which may be otherwise expressed by 5.74 of Smee's pairs.

Heat evolved by Combustion, when it terminates in the formation of an Electrolyte.

50. Finding that our information on the quantity of heat evolved by the combustion of metals was not very satisfactory, I have without wishing to depreciate the labours of Dulong, Despretz and others, thought it right to bring forward such of my own experiments as are necessary in order to make my investigation complete.

51. I provided two glass jars. The smaller had an internal capacity of 90 cubic inches; and when placed within the other jar, as represented by Fig. 7, the space left between the two was sufficient to contain three pounds of water. By means of a scale, *s*, suspended by wire from a thick fold of moistened paper, I was able to introduce a combustible within an atmosphere of oxygen, and by means

* Some experiments, which I have not time to refer to at present, render this hypothesis more than probable.

of a heavy weight I could keep the paper, sufficiently close to the top of the jar to prevent the escape of any considerable quantity of heated air, while at the same time it was not so tight as to prevent the admission of air as the oxygen was consumed. The increase of the temperature of the water was measured by a thermometer of great sensibility.

52. *The heat evolved by the combustion of zinc* was ascertained in the following manner. The smaller jar was filled with oxygen, placed in the other jar, and surrounded by three pounds of water, the heat of which was contrived to be as much below the temperature of the surrounding air as it was expected to exceed it at the close of the experiment. A piece of phosphorus, weighing 0.4 grain, was then put into the scale, and over it I placed a heap of fine zinc turnings, weighing 50 grains. I now ignited the phosphorus, and plunged the scale into the inner jar. After the combustion had terminated, and the heat thereby evolved had been evenly distributed throughout the water by stirring, the increase of temperature was noted. The contents of the scale were then thrown into dilute sulphuric acid, and the volume of hydrogen thereby evolved indicated the quantity (generally about 15 grains) which had not been burnt. Two-tenths of a degree of heat were deducted from the observed heat, on account of the phosphorus, and an allowance having been made, on account of the capacity of glass for heat, the results were reduced to the standard of one pound of water.

53. The mean of several experiments conducted in the above manner, showed that the heat evolved by the combustion of 32.3 grains of zinc is able to increase the temperature of a pound of water by $10^{\circ}8$.

54. *The heat evolved by the combustion of iron* was ascertained in a similar way. The iron was in the state of fine wire, and that portion of it that was not burnt was carefully collected, weighed, and deducted from the original quantity. The mean of several trials indicated that 28 grains could increase the temperature of a pound of water by $9^{\circ}48$.

55. *Heat evolved by the combustion of potassium.*—This metal, in pretty large lumps, was introduced into an atmosphere composed of equal bulks of oxygen and air. I then introduced a stout iron wire, sharpened at the end, into the jar, and with it I cut the potassium into small pieces. Under this treatment it soon became so soft, that every time the rod was lifted it would draw out a string of metal. In this state it often ignited, and the experiment was spoiled on account of the partial formation of peroxide. However, by careful management, I succeeded in making some good experiments, in which nearly all the potassium was converted into *potassa*; and the exact quantity of unoxidized metal was ascertained by observing the volume of hydrogen evolved when the contents of the scale were exposed to the action of water. The mean of these showed that the heat evolved by the conversion of 40 grains of potassium into *potassa* is able to increase the temperature of a pound of water by $17^{\circ}6$.

56. *Heat evolved by the combustion of hydrogen.*—The gas was burned in an atmosphere of oxygen, diluted with common air, by means of a jet furnished with a very narrow bore. A grain of hydrogen evolved as much heat as is able to increase the temperature of a pound of water by $8^{\circ}36$.

57. We shall now proceed to examine how far the theory of resistance to electric conduction agrees with the above experimental results.

58. We have seen (47), (48), and (49), that the intensities of the affinities which unite gaseous oxygen with zinc, iron, potassium and gaseous hydrogen, are as 1.32, 1.18, 2.05 and 1; and the proportional quantities of heat which were generated by the combustion of the equivalents of these bodies, are $10^{\circ}8$, $9^{\circ}48$, $17^{\circ}6$, and $8^{\circ}36$, or 1.29, 1.13, 2.105 and 1, a ratio which is very nearly the same as that of the intensities just given. Hence we see that *the quantities of heat which are evolved by the combustion of the equivalents of bodies are proportional to the intensities of their affinities for oxygen*. Now I proved in my former paper* that a similar law obtains in the voltaic apparatus, in consequence of its heat being produced by resistance to conduction. And hence we have an argument that the heat of combustion has the same origin.

59. But our proof of the real character of the heat of combustion is rendered more complete by regarding *quantities* as well as ratios of heat. From the quantity of heat generated by the motion of a given current along a wire of known resistance, we can deduce the quantities of heat which, according to the theory of resistance to electric conduction, ought to be produced by the combustion of bodies; and then these theoretical deductions may be compared with the results of experiment.

60. The mean of three careful experiments detailed in my former paper†, shows that if a wire, the resistance of which is an unit, be traversed by an electric current of $1^{\circ}88\text{ Q}\ddagger$ for one hour, the heat evolved by that wire will be able to increase the temperature of a pound of water by $15^{\circ}12$. Now I have ascertained experimentally, that a pair consisting of amalgamated zinc and platinized silver, excited by dilute sulphuric acid, is able to propel a current of $0^{\circ}168\text{ Q}$ against the whole resistance of the circuit, when that resistance is 5.2; consequently, a similar pair can propel a current of $0^{\circ}168\text{ Q} \times 5.2 = 0^{\circ}874\text{ Q}$ against the resistance which I have called an unit. But from (42) the intensity necessary to separate oxide of zinc into zinc and gaseous oxygen, is to the intensity of one of Smee's pairs as 3.7 : 1; consequently, the electricity produced by the union of zinc and gaseous oxygen must be sufficiently intense

* Philosophical Magazine, October 1841, S. 3, vol. xix, p. 275. (70.)

† Ibid. p. 266.

‡ I beg to remind the reader that my *degree*, expressed thus (1°Q), indicates that quantity of current electricity which, after passing constantly during one hour, is found to have electrolyzed a chemical equivalent expressed in grains; as, 9 grains of water, 36 grains of protoxide of iron, &c.

to propel a current of $0^{\circ}874 Q \times 3.7 = 3^{\circ}234 Q$ against an unit of resistance. Now, $1^{\circ}88 Q$, when urged against an unit of resistance, was able in one hour of time to increase the temperature of a pound of water by $15^{\circ}12$; therefore $3^{\circ}234 Q$ could, in the same circumstances, produce $\left(\frac{3.234}{1.88}\right)^2 \times 15^{\circ}12 = 44^{\circ}74$ of heat.

But in (70) of my former paper, I proved that the same quantity of heat should always (according to the theory which refers the whole of the heating power of the voltaic apparatus to resistance to the electric current) be produced by a given quantity and intensity of electrolysis, whether the resistance opposed to the current be small or great. Wherefore the heat, which on these principles ought to be generated by the combustion of 3.234 equivalents of zinc, is $44^{\circ}74$; or, in other words, *one* equivalent, or 32.3 grains of zinc, should generate heat sufficient to increase the temperature of a pound of water by $13^{\circ}83$.

61. Now, as I have before stated, the quantities of heat evolved by the combustion of the equivalents of bodies, ought, according to the theory of resistance to electric conduction, to be proportional to the intensities of their affinities for gaseous oxygen. These, in the cases of zinc, iron, potassium, and hydrogen, are 1.32 , 1.18 , 2.05 and 1 . Hence $13^{\circ}83$, $12^{\circ}36$, $21^{\circ}47$, $10^{\circ}47$ are the quantities of heat which ought, according to our theory, to be produced by the combustion of 32.3 grains of zinc, 28 grains of iron, 40 grains of potassium, and 1 grain of hydrogen.

62. By comparing these results of theory with the quantities of heat, $10^{\circ}8$, $9^{\circ}48$, $17^{\circ}6$, and $8^{\circ}36$,* which were (53—56) obtained from experiment, it will be seen that the former exceed the latter by about one quarter. Considering the difficulty of preventing some loss of heat, in consequence of the escape of air from the mouth of the inner jar (51) during the first moments of combustion, &c., it will, I think, be admitted that experiment agrees with the theory as well as could have been expected.

63. I conceive, therefore, that I have proved in a satisfactory manner that the heat of combustion (at least when it terminates in the formation of an electrolyte) is occasioned by resistance to the electricity which passes between oxygen and the combustible at the moment of their union. The amount of this resistance, as well as the manner of its opposition, is immaterial both in theory and in experiment; and if the resistance to conduction be great, as it most probably is when potassium is slowly converted into potassa by the action of a mixture of oxygen and common air: or little, as it probably is when a mixture of oxygen and hydrogen is exploded; still the quantity of heat evolved remains proportional to the number of equivalents which have been consumed, and the intensity of their affinity for gaseous oxygen.

* Crawford, whose method was well adapted to prevent loss of heat, obtained $9^{\circ}6$. More recently, Dalton observed about $8^{\circ}5$.

64. That the heat evolved by other chemical actions, besides that which is called *combustion*, is caused by resistance to electric conduction, I have no doubt. I cannot, however, enter in the present paper upon the experimental proof of the fact.

Broom Hill, Pendlebury, near Manchester,
October 5, 1841.

Memoir to serve as a History of the Combinations of Lead. By M.
J. PELOUZE.*

Oxamide and allantoine, in acting on the elements of water, by the intermediation of the alkalies, transform each other into oxalic acid and ammonia. According to M. Dumas, the first of these substances, for the discovery of which we are indebted to him, is formed of two radical compounds, which are, one part the oxide of carbon, $C^2 O^2$, and on the other part, amidogene, Az^2, H^4 .

M.M. Liebig and Wöhler, who have made us acquainted with the artificial production of allantoine, have not indicated what appeared to them to be its most probable formula, but it is clear that the composition $C^4 H^6 Az^4 O^3$, does not permit the supposition that it contains the oxide of carbon and of amidogene, which pre-existed there as in oxamide.

This observation agreeing but indifferently with the ideas generally adopted by chemists on the constitution of amides, and in particular that of oxamide, I have undertaken, on this latter substance, some experiments directed principally with the aim of uniting it with the metallic oxides, by causing it to lose its water, as M. M. Liebig and Wöhler have succeeded in accomplishing with allantoine. If, in effect, oxamide has suffered a loss of water in its union with the bases, this dehydration would be sufficient to shew that it contains neither oxide of carbon nor amidogene, since we know that in a state of liberty the oxamide is left represented without residue by these two binary compounds. I endeavoured then to unite the oxamide to the oxide of silver, and to the oxide of lead, but I could not procure this result. Under this relation the question of amides does not receive any new light, but in pursuing these enquiries I arrived at some novel results, which will form the principal object of my memoir.

A boiling solution of oxamide is not altered by the nitrate nor by the acetate of lead; but if we add to either one or the other of these salts a little ammonia, we shall soon see precipitated in abundance small white plates or laminæ, brilliant, and soft to the touch, which are formed of 90.5 of the oxide of lead, and 9.5 of anhydrous oxalic acid. It is a new degree of saturation of the oxalic acid, an oxalate of lead tribasic, $= 3 PbO, C^2, O^3$, in which the oxygen of the base and the

* *Comptes Rendus*, December 6th, 1841.

oxygen of the acid are in equal quantity, and which consequently corresponds to the oxalic acid crystallized in water.

The decomposition of oxamide in oxalic acid and in ammonia, occasioned without doubt by the insolubility of the new salt, is far more prompt here than with the aqueous alkalies and acids.

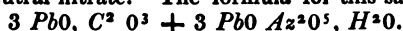
The tribasic oxalate of lead is formed also by pouring oxalate of ammonia into a solution of tribasic acetate of lead; but in this case it presents itself under the form of an amorphous powder, and without lustre.

This salt, prepared in any manner, absorbs carbonic acid from the air, and finishes by becoming transformed into a mixture of neutral carbonate and oxalate of lead.

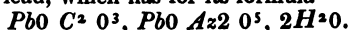
Acetic acid easily removes its excess of base.

The nitrate of lead is in the same case & boiled with this salt, it changes it rapidly into neutral oxalate, whilst that in its turn becomes basic.

When, instead of causing the oxamide to re-act on the ammoniacal nitrate of lead, in presence of a large quantity of water, we operate on concentrated liquors, we see deposited, even during the ebullition, granulous crystals, which received on a filtre, washed with cold water and dried in vacuo, are formed of tribasic oxalate of lead, united to a neutral nitrate. The formula for this salt is—



These same crystals maintained in the boiling liquor where they had birth, become changed gradually when this liquor contains neutral nitrate of lead, into another double salt, composed of oxalate and of nitrate of lead, which has for its formula—



It was presumed that these double salts, of which I have above spoken, were formed directly by the contact of neutral and tribasic oxalates of lead with the nitrate, under the condition of operating in liquors sufficiently concentrated. This is in fact what takes place.

The tribasic oxalate of lead, introduced into a boiling solution formed of one part of nitrate of lead and of two parts of water, changes into white crystals, bright and granulous, which are nothing more nor less than the first double salt formed by the combination of the tribasic oxalate, with the neutral nitrate of lead.

These same crystals are destroyed by a longer ebullition, and transformed into neutral oxalo-nitrate = $\text{PbO C}^2 \text{O}^3 + \text{PbO Az}^2 \text{O}^5, 2 \text{H}^2 \text{O}.$

The original water filtered boiling, leaves deposited on cooling, beautiful crystals, bibasic monohydrated nitrate of lead.

In all cases, the most simple means of preparing the latter of these double compounds, consists in making the two neutral salts which enter into the composition, act directly on each other.

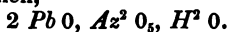
The neutral oxalo-nitrate of lead crystallizes in very brilliant hexagonal laminæ, insoluble in cold water, slowly decomposed by it in nitrate of lead, which dissolves it, and in the neutral insoluble

oxalate. Boiling water destroys this salt with the greatest rapidity. When submitted to the action of heat, it loses its water, and leaves disengaged, very soon after, corroding vapours mixed with carbonic acid.

Bibasic Monohydrated Nitrate of Lead.—The bibasic nitrate of lead was known long ago, but only in the anhydrous state. M. Peligot has shown that it may be obtained combined with an atom of water.

This salt is but slightly soluble in cold water ; much more soluble in boiling water, from whence it crystallizes with great facility in cooling. Carbonic acid decomposes it, and brings it back to its neutral state.

Dried in vacuo and raised to 100° , it does not loose the smallest quantity of water ; it is only at a temperature much more elevated, and comprised between 160° and 190° , that it loses its water of crystallization ; and even then, this dehydration takes place with extreme slowness. Towards 200° this salt becomes yellow, and sets at liberty a quantity of corroding vapours. At a red heat, it becomes destroyed completely, and furnishes $\frac{78}{100}$ of its weight of oxyde of lead. It has given from analysis 3.1 of water, and 19 of nitric acid. It has for its formula then,



This salt is formed under many circumstances, but there are none of them more worthy of notice, than that I have just spoken of.

On slightly heating a mixture of white lead, nitrate of lead and water, the whole mass soon becomes agitated by a tumultuous movement. It sets at liberty carbonic acid with great abundance, and with such rapidity that one would almost believe that there is free nitric acid in the liquid. This, filtered boiling, leaves deposited on cooling, a considerable quantity of bibasic monohydrated nitrate of lead.

The basicity of salt of lead never descends beyond this limit, however great be the excess of white lead. I am assured of this by the examination of the salt, and by the nullity of action of bibasic nitrate of lead on the carbonate of lead.

Placed in the same circumstances as the nitrate of lead, the neutral acetate produces nothing similar ; it does not disengage the carbonic acid of the ceruse, and preserves itself intact.

The tribasic oxalate of lead, boiled with a large quantity of water, and with an excess of nitrate of lead, becomes changed into neutral oxalate, and in its turn the neuter nitrate becomes bibasic hydrated nitrate.

On the other side, when we boil a solution of the tribasic acetate of lead with an excess of the neutral oxalate, the filtered liquor is no longer troubled by the carbonic acid, a circumstance which shows that the former salt has ceded $\frac{2}{3}$ of its base to the latter.

Sub oxide of Lead.—M. Dalony has announced that in decomposing the oxalate of lead by heat, we obtained a black amorphous powder,

which he considers as a new oxide less rich in oxygen than the protoxide or litharge. M. Boussingault repeated the experiments of M. Dalony, extended them and arrived at the same conclusions as he did. Most chemists, still leaning towards the opinion of our honourable colleague, have regarded the question as being not yet resolved. Some of them even, and particularly M. Winkelblek, deny the existence of a sub oxide of lead, and regard it, either as an atomic mixture of lead and its protoxide, or even as a variable mixture, though an intimate one, of these two bodies.

In again searching for the cause of this diversity of opinions, I have believed it possible to attribute it to the diversity of the products themselves, which ought to give oxalate of lead, by supposing that its decomposition by heat has been made at unequal temperatures. The results at which I have arrived confirm me in this idea.

Since then I have applied to the decomposition of the oxalate of lead the same rules which I made known in 1833, on the occasion of the pyrogenous products of tannin, and the result has been such as I expected.

The oxalate of lead, heated in a cucurbit placed in an oil bath, manifested signs of decomposition at a temperature bordering on 300. The temperature was then maintained as stationary as possible.

The gases are disengaged with a great degree of slowness. They consisted of carbonic acid and oxide of carbon. They were collected and analysed throughout the whole duration of the operation, which was very long. They constantly presented the exact relation of 75 to 25, or 3 to 1. Toward the end only, when, in order to terminate the operation it became necessary to raise the temperature a little, the proportion of carbonic acid was a little augmented.

This relation of 3 to 1 between the carbonic acid and the oxide of carbon gases, is the same as that indicated by theory, by admitting that the fixed product of the cucurbit is a sub oxide, having for its formula Pb^2O , or a mixture of equal atoms of lead and oxide of lead.

In fact $(PbO\ C^2O^3) = Pb^2O$, or $Pb + PbO + C^4O^7$, which becomes translated into $C^3O^6 = 6$ volumes of carbonic acid, and $CO = 2$ volumes of oxide of carbon.

If we heat directly over charcoal, or the flame of a spirit lamp, the cucurbit which contains the oxalate, and which is the same as was done by M. Dalony, Boussingault, and Winkelblek, we have control no longer over the temperature, and the gases vary perpetually in their relations; a fact which indicates complicated decomposition.

The sub oxide of lead, prepared as I have said, and after having taken all the possible precautions to avoid it having contact with the air, is a compound perfectly definite. It is of a well grounded black, sometimes dull, sometimes rather soft like velvet. It does not contain metallic lead, and I prove it in a decisive manner, and at the same time very simply; by boiling it screened from contact

with the air, with a solution of cane sugar, which dissolves the litharge very well, and absolutely takes nothing from the sub oxide of lead.

The nitric, sulphuric, hydrochloric, and acetic acids, feeble or concentrated, do not form distinct salts with the sub oxide of lead; they change it into metallic lead, very finely divided, and into ordinary oxide, with which they combine.

The soluble alkalies act in the same manner. The nitrate of lead itself changes it into protoxide of lead and metallic lead. It disappears in a diluted solution of this salt, and the liquor filtered boiling, leaves deposited a mixture of nitrate and of basic nitrate of lead.

Mixed with a small quantity of water, in contact with the air, the sub oxide of lead acts in a remarkable manner, and of which we do not find any reasonable explication, except by admitting that it really constitutes a definite compound.

It then produces much heat, rapidly absorbs the atmospheric oxygen, and becomes converted into a white powder, which is the ordinary hydratic oxide. A mixture of lead, very finely divided, and litharge in a fine powder, produces nothing similar.

Heated to a dull red the sub oxide of lead becomes decomposed, and forms a mixture of lead and its protoxide. We recognise this decomposition with facility, whether by amalgamation, or with boiling sugar water, which dissolves the oxide, or with feeble acetic acid, which leaves a residue of lead, which in place of being in a fine powder, as with the sub oxide, presents itself in net work, which needs only to be compressed between the fingers to become a compact mass of a metallic aspect.

Besides, the mixture is immediately distinguished from the sub oxide by its colour, which is yellow, slightly tinged with green.

The tribasic oxalate of lead is decomposed by heat, like the neutral oxalate; but the gases vary in their relations during the whole length of the operation, and the residue is a mixture of the sub oxide and of protoxide. I was assured of this upon using a boiling solution of sugar, which dissolved a considerable quantity of the protoxide of lead.

The analysis of the sub oxide of lead has not in any manner to be made after the examination of the relation of the gases proceeding from the oxalate.

This relation sufficiently indicates for the sub oxide the formula Pb^3O , which likewise confirms the properties of this compound.

100 parts of sub oxide heated in contact with the air, have given 103,7 and 103,6 of the protoxide.

This oxidation is effected with great facility, for the sub oxide of lead is pyrophoric, and when we heat it in a weight of its mass, the whole mass entire takes fire.

The incontestable existence, however, of the sub oxide of lead is important for the general history of the combinations of oxygen with the metals. This sub oxide will doubtless not remain long the only one of the species, but it will soon place itself at the head of a

series of compounds which will present, with the superior degrees of the oxidation of the metals, this very singular point of resemblance, that neither one nor the other can combine with the acids.

The oxalate of zinc gives from the dry distillation, the ordinary oxide of zinc, and sensibly too, equal volumes of carbonic acid gas and oxide of carbon.

That of copper is decomposed with the greatest facility ; it sets at liberty carbonic acid almost pure, and gives a residue of copper quite metallic, in scarlet spangles, brilliant and malleable, some of which are several millimetres broad, although they certainly proceed from a pulverulent and amorphous matter, which has not been submitted to any fusion.

On the Theory of the Fabrication of Carbonate of Lead (ceruse).
—The process for the fabrication of white lead, by M. Thenard, is well known, and was for the first time executed by M. Rouard, in his domestic economy of Clechy. This process, known under the name of the *French process*, to distinguish it from another kind of fabrication employed at first in Holland, consists in making carbonic acid pass into a solution of tribasic acetate of lead. This latter salt cedes to the carbonic acid the two of its base, which it deposits in the state of white lead, and thus becomes neuter. It may serve anew for the same operation, after its having combined directly with the oxide of lead. We conceive that a considerable quantity of ceruse may thus be produced, by a comparatively very feeble proportion of acetate of lead, and consequently by acetic acid. There would even be no limit to the production of ceruse with the same vinegar, if this salt did not retain a feeble quantity of acetate of lead.

A modification has been projected in England of the process of M. Thenard, by which they have, if I may so express it, transformed it into a process *by the dry method*. This process consists of mixing with litharge about the hundredth part of its weight of acetate of lead, and to cause to pass on the mixture, previously wetted with a very small quantity of water. In a few hours all the litharge is carbonated, and the operation is finished.

Carbonic acid and oxide of lead, alone, are only united with extreme slowness. It is necessary then to admit, that the some thousandth parts of acetic acid, which are found in the preceding mixture, are conveyed over the entire mass of oxide of lead, to constitute a basic ascetic, which is destroyed and re-produced without cessation.

That process named the *Holland one*, which was transported many years ago to Lille, where it has become a very important object of industry, consists in exposing the plates of lead to the vapour of vinegar and to the exhalation arising from horse dung. The vinegar made use of is that made from beer of an inferior quality, and which contains a very small quantity of real acetic acid. After an examination which I made of this vinegar, for which I am under considerable obligations to the courtesy of M. M. Lefevre and Decoster, manufacturers of ceruse at Lille, the weight of the real

acetic acid does not amount to the one hundred and fiftieth part of the weight of the lead, and we know that in good operations almost the whole of the lead is transformed into ceruse. M. Graham has arrived at similar conclusions in England: he has even found less acetic acid than I have, relatively to the weight of the lead.

It is impossible then that the carbonic of ceruse proceeds from the decomposition of the vinegar.

The manufacturers, on the other hand, well know that they cannot obtain ceruse except they establish with care currents of air in the mixtures above spoken of.

The theory of this fabrication is very simple then, and similar to that of the other processes of which I spoken in the first place.

The oxidation is caused by cold air, and the vinegar, in vapourising under the influence of considerable heat produced by the fermentation of dung, unites with the oxide of lead, from whence it is soon displaced by the carbonic acid disengaged in abundance by the dung. In the unwashed Holland ceruses, a great portion of the acetic acid is recovered.

I believe that such is the manner in which these things take place; and during the ten years since I have left Lille where I was enabled to study this fabrication, I have always presented this theory as being the most rational. At this epoch, almost every chemist thinks that the carbonic acid co-operates with its elements to the composition of ceruse.

I have made an experiment which shows plainly the part which vinegar takes in the fermentation of ceruse. I composed an artificial atmosphere of oxygen and carbonic acid, and I abandoned to itself in this atmosphere, a plate of lead placed over a vase containing vinegar. At the end of three months, the plate of lead had become converted into a white crust of ceruse. The proportion of this was such as to indicate that the oxygen and the carbonic acid were absorbed. Almost the sum total of the vinegar was found. The proportion which had served to determine the formation of the ceruse was so small, that it could not be estimated.

Another very curious experiment shews very well, in my opinion, the true part which the acetic acid acts in the formation of ceruse, and the necessity of introducing in this fabrication an acid susceptible of producing with the oxide of lead a sub salt decomposable by the carbonic acid. If in the preceding experiment we substitute formic acid for the vinegar, which does not produce, as is well known, the basic salt with the oxide of lead, it does not form ceruse, even after several years of contact between the vapours of the formic acid, the metallic lead, and the oxygen and carbonic acid gas. Formic acid, nevertheless, is very nearly allied, by its affinities to acetic acid, and volatile almost to the same degree as it, but it does not form the basic salt with the oxide of lead, and the neutral formiate of this metal is not decomposed by carbonic acid; this is the reason why its use is improper for the production of ceruse.

Atmospheric Electricity.—By MR. THOMAS SPENCER.

SECTION V.

In every branch of human knowledge, not even excepting the strictly scientific, the propounder of a theory, among the disadvantages he has to contend with, and they are not few, runs the risk of compromising any reputation he may have hitherto gained, as well as of throwing a bar in the way of the due appreciation of future labours, although, perhaps, of unquestioned utility. Such are the general results, in the event of his views proving ultimately to be correct; one generation having to pass away, and himself with it, before they are admitted to rank with the elementary knowledge of the next. On the other hand, should they be incorrect, the consequences are disastrous, and a long life of future scientific labour is often insufficient to compensate for the one blunder.

With both these positions fully before me, I have ventured to propound my views *theoretically* on the subject of atmospheric electricity; but have hoped the strong facts on which they are founded will at once render them obvious even to the unscientific, and, perhaps, spare me much of what I have just predicated.

While these papers have been undergoing publication, I have received several communications on the subject from different quarters; some giving their assent; others blaming my rashness and withholding assent; and a third requiring further information, the published papers being necessarily abridged. To the latter I have replied privately; but, with respect to those who dissent in opinion, they hold my grand error to be, that I have assumed a *chemical* action to have taken place when water has been converted into steam, and a like action when the steam is reconverted back to water. Another objection, less easily answered, is made, that I have taken the imponderables, heat and electricity and used them as so many *ponderable* bodies; or, in other words, that I have taken two things that are known to exist universally in nature, but are not recognised to have *substance*, and are merely considered under the vague title of *forces*, that have no weight, and used them as chemical compounds.

To this I plead guilty, in the fullest sense of the word. Yet, be it observed, I still look on these bodies as imponderable—that is, *relatively to the mass of our planet*. But that they are imponderable, relatively to the mass of the centre of our system, I cannot admit. In relation to that body, they may have sufficient density to be atmospheric; indeed, unless some such state of things have existence with respect to those bodies, in relation to the sun, it is difficult to conceive how the principle of gravitation can be universal.

Hydrogen, the lightest of known bodies, on the surface of the sun would weigh twenty-nine times more than it does with us; while, at the surface of our own satellite, no reasonable doubt can exist that its ponderability would be hardly detectable by the finest conceivable

mechanism, and *there*, would consequently be ranked among the imponderables : yet *we know* its ponderability.

Voltaic, or more properly, chemical, electricity has been already considered by Dr. Hare as a compound of another species of electricity with caloric. How far he carries his opinion, I am, at present, unaware : but, at the hazard of being deemed speculative, I may venture to say that we are borne out by analogy in supposing that the four recognised species of electrical power, namely, chemical, frictional, magnetic, and thermo-electricity, are, in all probability, compounds of one base, combined *atomically* with caloric in different proportions.

Speculations regarding the nature of electricity are daily becoming more interesting, although, perhaps, of questionable utility ; but when we take into view the fact, that every form of matter we are acquainted with is saturated in definite proportions with this wonderful power, lying, as it were, dormant, but never, under any circumstances, refusing to be drawn forth to perform its functions when the proper means are applied ; and when we recollect, that, like substances, it can never be dissipated,—the force, it is true, may be misapplied by ignorance, but not wasted,—fulfilling its assigned office during its transit from one body to another, from whence it may be again and again recalled with definite, yet undiminished vigour.

With these facts before me, surely I may be pardoned in looking on electricity as *something more* than a mere force in nature, *without substance*. Dissent from these opinions does not, however, necessarily invalidate, or otherwise, what I have already promulgated on the nature and proximate origin of the electricity of the atmosphere. On the contrary, had other proofs not existed, I should never have hazarded them.

The experimental proofs of the necessary nature of cloud arise out of the following fact, discovered accidentally at the close of 1840, and since followed up with much ingenuity by Mr. Armstrong, of Newcastle, and confirmed by many others. *This is the electricity that is found to exist in connection with steam.*

I now quote from a communication made by Mr. Armstrong to Dr. Faraday. After describing the portion of the boiler, &c., in which the occurrence took place, he says, "About three weeks ago the steam began to escape at this joining, through a fissure in the cement, and has ever since continued to issue from the aperture in a copious horizontal jet. Soon after this took place, the engineman, having one of his hands accidentally immersed in the issuing stream, presented the other to the lever of the valve, with a view of adjusting the weight, when he was greatly surprised by the appearance of a brilliant spark, which passed between the lever and his hand, and was accompanied by a violent wrench in his arms, wholly unlike what he had ever experienced before. The same effect was repeated when he attempted to touch any part of the boiler or any iron-work connected with it, *provided his hand was exposed to the steam*. He next found, that, while he held one hand in the jet of steam, he

communicated a shock to every person he touched with the other, whether such person were in contact with the boiler, or merely standing on the brickwork which supported it; but that a person touching the boiler received a much stronger shock than one who merely stood on the bricks." Mr. Armstrong made some further experiments, all of them tending to confirm his first observations; but, being unable to account for these appearances, he committed their results to Dr. Faraday. This gentleman suggested a number of experiments to Mr. Armstrong; and, in his remarks on the nature and origin of electricity discovered to exist in steam, says, "The evolution of electricity by evaporation described by Mr. Armstrong is, most likely, the same as that already known to philosophers, but on a much smaller scale, and about which there are, as yet, doubts whether it is to be referred to mere evaporation, as Harris says, or to chemical action, according to others. This point neither settles nor illustrates; but it gives us the evolution of electricity during the conversion of water into vapour upon an enormous scale, and, therefore, brings us much nearer to the electric phenomena of volcanos, waterspouts, and thunderstorms than before." The electricity alluded to by Dr. Faraday as being already known to philosophers has, in reality, as much relation to the electricity proceeding from the conversion of steam into water, as light has to darkness, inasmuch as the negative state was already known, or, more properly, suspected, when water was being converted into steam, but the positive electricity discovered on this occasion was never before detected under the same circumstances.

Positive electricity is the *only* electricity, negative electricity being the appellation given to a state of things where *it does not exist*, or just as we speak of *cold* as being the opposite of *heat*.

The proofs we have of the existence of that peculiar state of things termed negative electricity, in connection with evaporation, are illustrated in the following simple experiment:—Adjust a platinum crucible, containing a little water, on the brass ball or plate disc of a common gold leaf electrometer, then add a red hot cinder to the water contained in the crucible, which will cause evaporation to take place, and, at the same time, the gold leaves will be distended towards the sides of the instrument. This action of the leaves points out to us that when water is being converted into steam, in other words, evaporated, that electricity is *absorbed*. Not only is electricity absorbed during the progress of evaporation, but I am led to infer further, that it is essential to the process itself. Since this paper was read in Liverpool, a paper has been published in the *Philosophical Magazine* detailing an experiment which goes very far to prove this. The writer, Mr. Rowell, took two cups of water, each containing eight ounces. One of them was made to communicate with the earth by means of a copper wire: the other was what electricians term insulated, having *no* metallic communication with the earth. They were both placed in otherwise equal circum-

stances in a warm room over an oven. In twenty-five hours the cup, having electrical communication with the earth, had evaporated fourteen dwt. and nine grains *more* than the one having *no* communication. I have repeated a similar set of experiments, and with nearly similar results, always, at least, showing an excess in favour of non-isolated vessels of water. I may here state that the writer, in the same communication, briefly says, he has entertained views somewhat similar to some of those I have published in the foregone sections, and that he has entertained them since 1840. As he will, in all probability, publish them in detail, at present I can only say my own views on the same subject were far from being matured until a later period.

In the paragraph above quoted from Dr. Faraday, it is also worthy of remark, that he at once seemed to perceive the necessary connection that existed between the electricity given out by the steam and the electrical phenomena of the heavens.

In the first experiments made by Mr. Armstrong, he was led to imagine, that, if electricity was given out with the steam at a small aperture of the boiler, it would, in all probability, exist in much larger quantity in the interior. Of course, with the views I have explained, namely, that its existence in the free state was caused by the conversion of the steam into water, it was not difficult to foresee, that any experiments to prove this would fail. This gentleman, however, made a series of experiments with this view, but *no* electricity could be elicited in the steam, *while it was steam*, but only when it had become water was this force perceptible.

From the results of these experiments, it is scarcely possible not to view them as principles that may not prove unimportant in the great business of life, but more particularly with reference to the economy of steam-engines. We thus see evaporation, or the formation of steam promoted by metallic conducting substances, on the one hand, and, when about to be condensed, on the other; there can be little doubt that a like conducting substance to carry off the electricity generated during the process would also facilitate it. 'I have no means of making such an experiment on the proper scale; but, where even a small saving of fuel might be the result, it is worthy of consideration, and I should be but glad to hear of such experiments being made.'

With respect to the application of these results to the electrical phenomena of the atmosphere, I trust they are, by this time obvious. Still, I may be permitted to repeat, that the air, at all times, contains a very large proportion of steam, invisible to the eye, and, hence, termed transparent aqueous vapour. From the water contained in this steam is formed cloud, rain, hail, and snow. The accidental result observed in the steam-boiler, at Newcastle, shows us clearly, that, when steam is condensed, it gives out electricity. Cloud, then, is admitted on all hands to be the condensed steam of the atmosphere. I now ask, will it not give out electricity? or, I may add, is this phenomenon to be peculiar to the immediate sur-

face? or is it not far more likely to be collected in greater quantity in high altitudes, where the process of cloud-forming goes on unconnected with any conducting substances, where it is, in fact, insulated?

This granted, there remains not the smallest difficulty in accounting for the hitherto mysterious origin of electricity in the atmosphere. When we look at the immense amount of evaporation going on at all times on aqueous surfaces, and the corresponding amount of condensation going on in the atmosphere, it is, at first sight, only wonderful that the resultant electricity does not play a more conspicuous part than it really does. The observing inhabitant of a tropical climate will have much less difficulty in giving his assent to these views than he of a colder or more temperate one.

There are many facts and observations that might be adduced of the *essentially* electric nature of cloud, which, from the nature of the medium in which these papers have been published, are, necessarily, omitted; but I may mention, that luminous falls of snow have been observed from time to time. Those are perfectly explainable on the principles published in these papers. The most notable was observed by the Rev. Colin Campbell, and published in the first series of the *Edinburgh Philosophical Journal*. I may also mention that, when relating my views on atmospheric electrical phenomena, some months ago, to a celebrated professor of astronomy, he informed me, that, while observing the movements of the heavenly bodies on clear, star-light nights, it sometimes happens that a cloud will form often obstructing the field of view. Under those circumstances, he has been able to note the time, by his chronometer, with the naked eye, which, while there was no cloud visibly existing, he was unable to do. Such an occurrence is perfectly explainable, if we suppose a mass of cloud each globule of which shall be surrounded by an atmosphere of electricity, there would be luminosity enough emitted to account for increased light.

I now close my observations on these subjects; and I may add, that I have endeavoured to use as few really technical terms in explaining my views as the subject would admit. I have used the medium of a newspaper to convey them, in the first instance, to the public; a mode of conveyance of scientific subjects generally sneered at by contributors to philosophical journals, as they affirm, that an appeal to usual newspaper readers is made to those least capable of judging on a scientific subject. There is some force in this, and, with me, would have had weight in more abstract science. But whatever relates to meteorology, I hold, cannot be too widely diffused in a great maritime town like Liverpool, the subject now being admitted to be of immense importance, since the science has become more fixed, and less subject to quackery than it was a very few years ago.

If these papers should have the slightest effect in tending to increase the desire for meteorological observation in Liverpool, I should feel much gratification. Public meteorological observatories should be general throughout the empire; but in no place that we

can name is the want more felt than in our own town. Occasional astronomic observations for navigation are practically useful; further than this, their practical utility is questionable. But, with meteorological science, every observation has a practical tendency, making us more completely acquainted with the changes that take place in the sea of atmosphere that surrounds our earth, and in which reside the elements of *all* those changes usually termed *weather*, good or bad, hot or cold, damp or dry; and, it may be justly added, on its changes depend the existence of much health or disease; prevalent epidemics being attributed, by many, and perhaps justly, to atmospheric causes.

POSTSCRIPT.—In the preceding papers a few errors have crept in, especially in section the fourth, where “caloric of *electricity*” occurs, it should be “of elasticity,” also for “most *elastic*,” respecting water, read “most *inelastic*.”

In propounding these views, it will be observed I have altogether assumed the theory of Franklin respecting positive and *negative* electricity, believing, in point of fact, the latter to be a nonentity; but, where I make mention of negative bodies in relation to clouds, it is meant in relation only to cloud possessing a *larger* portion of the subtle agent.

LONDON ELECTRICAL SOCIETY.

ON Tuesday, July 21st, 1841, a paper was read by Andrew Crosse, Esq., the subject of which was “The Perforation of non-conducting substances by the mechanical action of the Electric Fluid.”

The author states that about some twenty-two years ago, he found that if an intercepted current of electricity, from a common electrical machine, were passed between the points of two platinum wires, well screwed on a narrow slip of window glass, both wires being on the *same side* of the glass, and in the same straight line, the vicinal extremities about one tenth of an inch apart, and the remote extremities in connection with the positive and negative conductors respectively, and that if the strip of glass thus prepared were placed in a vessel of water, it would become perforated, by the sparks passing between the points of the wires, from about 200 or 300 revolutions of the machine. The perforation is cylindrical, and tolerably even from one side of the glass to the other, and at right angles to its plane. Another person who had repeated the experiment found the glass perforated beneath the point of the *negative* wire, and not directly between the two wires. This experimenter considered the perforation due to chemical, and not mechanical, action; resulting, as he thought, from a decomposition of the glass. Mr. Crosse afterwards repeated the experiment many times, and in every case the perforation was directly *between* the wires, and is perfectly satisfied that the effect is mechanical; and states that the most

powerful electrical machine would fail to produce a visible decomposed speck on glass however exposed to its action.

About two years ago Mr. Crosse's attention was again directed to this experiment, and was led to vary it in the following manner. The strip of window glass was four inches long, and half an inch broad; and to one side of it the platinum wires (one-thirtieth of an inch diameter) were secured by tying with waxed silk thread, having their vicinal extremities one-twelfth of an inch apart. The glass thus equipped was placed in an empty dish, with the wired side upwards, and the remote extremities of the wires connected with the prime and negative conductors, as before: with the precaution of intercepting the current and forming it into distinct and powerful sparks, by placing one or two insulated brass balls at the positive end of the circuit. By this arrangement no effect was produced on the glass by one hundred revolutions of an eighteen-inch cylinder machine, in high working order, and capable of producing a fifteen-inch spark between two five-inch balls. 2nd. The dish in which the glass was placed, was next filled with water, covering the points of the wires to the height of an inch. Twenty-five revolutions of the machine produced a fracture in the glass, between the points of the wires. One hundred revolutions extended the fracture, and produced two small cracks which diverged from the positive to the negative side. Two hundred revolutions occasioned a small excavation between the wires, but on the *opposite* side of the glass; and by two hundred and fifty revolutions, the perforation was completed. 3rd. Another slip of window glass was prepared as in the last case, but the wires were only one-twentieth of an inch apart. A perfectly circular hole, without any crack, was made through the glass *between* the wires, by three hundred revolutions of the machine. The hole in this case, as in the last, was conical, being narrowest on that side on which the wires were placed. If the water barely covers the wires on the glass in experiments of this kind, it is projected upwards from between the wires to more than a foot high, playing like a fountain by the *lateral* force of the sparks. If the glass be inverted in the dish, having the wires on the lower surface, and the whole barely covered with water; the electric sparks, as they pass between the wires, displace minute pieces of glass from the *upper surface* of the slip, and a small jet of vitrious particles is kept playing upwards, as was the water in the former case. 4th. Two platinum wires were secured at one-twentieth of an inch apart by wax, to the bottom of the inner surface of a watch-glass and the latter filled with water; twenty revolutions of the machine perforated the glass, which first yielded on the *outside*. 5th. Two wires were fixed, as in the preceding cases, to the outside of a glass tumbler filled with water. No effect was produced by 100 revolutions. The wires' points were afterwards covered with wax, and 200 revolutions produced a slight abrasion of the glass between the wires, but

no effect was produced inside of the tumbler. 6th. The wires were fixed between two strips of glass well tied together, but no effect was produced by 100 revolutions of the machine; but by placing the wires and glass under water, twenty revolutions produced an effect upon each piece of glass, and 150 revolutions split one of them to pieces. 7th. The wires were next fixed to a piece of plate glass one-eighth of an inch thick, and placed under water. By 500 revolutions it was pierced to about one-third of its thickness, on the wired side; the other side not being affected.

Mr. Crosse observes, that although a regular stream of fire apparently passes between the metallic points, in reality it is a succession of small explosions, at each of which, the water is forced in one direction, and the glass in another; and also the wires have a strong tendency to be separated from the glass to which they are attached; and the effect on the glass between the points is proportional to the contiguity of the wires to the glass, as well as to the strength and number of the successive small explosions.* If the wires exceed the distance of one-twentieth of an inch from each other, the electric force necessary to leap across the interval will often break the glass, and at all events pierce an irregular hole. The wires also have another tendency, which is to unite and slightly solder together, even under the water. It is therefore absolutely necessary that they should be perfectly secured in their places. When the glass is thin, a series of vibrations is communicated to it, sufficiently powerful to separate a small portion of its substance from the *opposite* side; but when plate glass is used, this effect does not take place; apparently on account of its not being susceptible of being thrown into similar vibrations; and as it presents a stronger resistance, the effect is produced on the same side on which the wires are fixed.

Mr. Crosse next fixed the wires very firmly on the side of a perfectly transparent crystal of quartz, at a distance of one-twentieth of an inch. After 100 revolutions, a small mark was visible between the wires, but the surface of the crystal remained smooth. I then immersed it in a glass dish of water, and passed 100 revolutions across it. On examination I found a very sensible excavation between the wires, on the same side of the crystal. After 500 revolutions it was pierced to a much greater depth, and with a much larger excavation than the glass in the last experiment. This increased effect was produced by the wires having been more firmly secured on the surface of the crystal. I am inclined to think, that with a proper apparatus, well contrived to secure a firm pressure of

* Those readers who have perused my *fourth memoir*, "Annals of Electricity, &c.," vol. iv, p. 178, will find that several of my statements are in perfect accordance with the results of Mr. Crosse's experiments. We have the *lateral* effects of these miniature *explosions*, perforating solid bodies, throwing up water, and even separating the wires from the glass, in the manner that I showed lightning conductors would be peeled from the mast of a ship.

both wires on the substance experimented on, and by a proper regulation of the distance of the extremities of the wires, together with a sufficiently powerful stream of electricity, two or three times interrupted, not only glass may be sufficiently, easily, and readily drilled, but even the diamond and other gems may be perforated in the same way.

ELEMENTARY LECTURES ON ELECTRICITY, ETC.*

LECTURE XVI.

The experiments shown on strips of gold or silver, may be varied by employing their wires instead of metallic leaf. For this purpose the thinnest watch pendulum wire that can be procured is exceedingly useful, as it can be fused by the discharge from a moderate sized jar.

We will first discharge a large jar through a piece of this steel wire about ten inches in length, and you will find that the wire is barely made visibly red hot, although the intensity of the charge was tolerably high. Now the reason of our not succeeding in fusing the wire by this discharge, was not because of its being too thick, but simply because it was too long; and in order to convince you of this fact I will shorten it by one half, leaving a length of five inches only to be traversed by the next discharge of the same jar; the intensity of the charge being the same as in the former case. The result is as was predicted, the wire is fused, and you will have observed also, that the mechanical action was so great as to scatter the globules of the fused steel to a great distance on every side. We will presently make a similar experiment on a larger scale, but I must first vary it with the same jar.

Now in varying the experiment I will simply introduce a piece of wet thread to the circuit, in order to abate the mechanical action of the discharge, as on a former occasion. In this experiment I will operate on the five inches of wire which was cut off from that employed in the last experiment. Now, observe attentively, that the intensity of the charge is quite as high as before, indeed, somewhat higher; but the visible effects on the wire, by the discharge of the jar, are simply a few tremulous motions, and not the slightest indication of redness from an elevation of temperature are perceivable. Here again is another proof of the influence of electro-mechanical action in accomplishing the fusion of metallic bodies by ordinary electric discharges.

When experiments of this kind are carried on for the purpose of ascertaining the effects of certain electric charges, on wires of different

* The last paragraph but one of lecture XV, p. 253 ought to have read thus. "I know of no other experiments by means of which I could show you, to greater advantage, that the calorific effects on the gold depend on the electro-mechanical action of the discharge."

dimensions, the balance electrometer of Cuthbertson (see p. 250), will be found very convenient. There is also another circumstance to be noticed in experiments of this kind. If the air within the jar be perfectly dry, the quantity of electric fluid that it will contain will be somewhat less than if the air were a little moistened, either by breathing into it, or by any other means.

Let us place the bent arm of the balance electrometer in connection with the prime conductor of the machine, the inside of the jar also being in connection with it. I will place about two inches of watch pendulum wire between the insulated ball of the electrometer, and the outside of the jar; I will also adjust the sliding weight on the graduated arm of the instrument to fifteen grains. Now, it will be obvious that when the charge has arrived sufficiently high for the repulsive action between the balls *p* and *c*, to overcome the load of fifteen grains, that they will separate by the upward motion of the ball *c*; and when the ball *p*, at the other extremity of the balance arm, has, by its descent, arrived sufficiently near to the insulated ball *q*, as to be within the *striking distance*, the discharge will take place, and the contents of the jar will traverse the steel wire. These arrangements being complete, and the route of the electric fluid being understood, we will now proceed to the experiment. A few turns of the machine causes the ball *c* to move, and a few more, you will observe, accomplishes the discharge. The steel wire is not only made red hot, but is fused, and partly scattered in small globules.

If now, we were now to add another inch to the length of the steel wire, and transmit through it a similar discharge to that employed in the preceding experiment, we should find that the wire would become just red hot, but not fuse; and to satisfy ourselves on this point, it is necessary to repeat the experiment two or three times. This being done, and still the wire remains perfect, so that it is pretty certain a discharge measured by fifteen grains is not sufficient to accomplish its fusion. We will now slide the weight to twenty grains, and having charged the jar to that point, the discharge through the steel wire immediately accomplishes its destruction.

Now in order to show that a jar will hold more of the electric fluid when moist within, we will first try how long a piece of wire can be fused, by the highest intensity of the jar when dry; and afterwards moisten the air within the jar, by breathing into it, and the experiment will prove that the discharge from the moistened jar, will fuse a much longer piece of wire than one from the dry jar.

When the coated surface of glass is very extensive, as in a battery of jars, for instance, this class of experiments can be exhibited on an extended scale, and with the most brilliant results. If for instance, we were to connect the inside of the battery (p. 176) with the prime conductor, and place the sliding weight at about forty grains, we should be enabled to fuse thin steel wire above forty

inches in length. To convince you of the beauty of this class of experiments, I will place a piece of iron wire, two feet long, in the circuit. Now observe attentively what takes place. Did you ever behold such a splendid sight? The wire, you will have observed, is first heated to the brightest redness; progresses perceptibly to an intense white; swelling in thickness to six or eight times its original dimensions, and eventually, it bursts into thousands of minute balls which became projected in every direction, and fell in a shower of fire.

The beauty of this experiment is considerably enhanced by coiling the iron wire into the shape of an open helix, like a cork-screw. You will observe that when the discharge has passed through the helix, the latter retains its shape for a perceptible time, more than a second for instance, before it bursts; which done, it is dispersed into a multitude of fiery globules.

By transmitting similar discharges through wires of platinum, gold, silver, copper, brass, &c., their fusion is very readily accomplished; and when very thin they are converted into a cloud of smoke, which becomes dispersed on every side in the surrounding air. Now, by making a series of experiments on these different metals, whilst suspended in the air, and in a darkened room, we shall see that each metal, during fusion, yields a peculiar coloured light. The platinum wire, you will observe, yields a whitish light, not very intense. The gold yields an orange-coloured light; and the silver a beautiful yellowish green light, of considerable intensity. The light proceeding from the copper, is of a much deeper orange than that from gold, but the brass yields scarcely any light whatever.

If such a series of wires were to be exploded between cards, those cards would be stained by different colours: and you will observe, by a few experiments, that whatever be the character of the wire, the deepest stain on the cards between which it was placed is precisely in its own direction: and that the intensity of the stain becomes more and more attenuated from the axis of the discharge to the margin of the cards. Moreover, by a close examination of those cards, especially those between which brass, silver, or gold were exploded, you will perceive that a complete gutter is made in each card, directly opposite to the axis of the discharge, or the direction of the wire.

The stain made by brass is nearly black, and the figure is much in resemblance of a caterpillar. That from silver is of a greyish colour, and much scattered; gold yields a purple or violet coloured stain, much scattered, and the tints beautifully blended. Iron and platinum give no very striking colours, nor do they stain the cards to any great extent. They seem to be fused into a train of minute globules, which are left on the cards in the directions of the original wires. When iron wire is exploded between two pieces of glass, a series of minute globules of iron are firmly fixed on the surfaces of each piece, and stand very prominently above the surface of the glass.

On Electro-Gilding.—By Professor DE LA RIVE, of Geneva.*

Having long observed the baneful effects of the mercury employed in the ordinary mode of gilding, it occurred to me that the decomposing powers of an electric current, when applied to a solution of gold, might, by bringing that metal gradually upon the surface of the object to the gilt, be substituted, in many cases, if not in all, in the place of the mercury; and it is now fifteen years ago since I first made experiments on this inquiry. These first trials, however, not being successful, I dropped the investigation for the time. The progress which, since that time, has been made in electricity, and especially the discoveries of M. Becquerel, have induced me to resume the inquiry, and conduct my experiments in a somewhat different manner to those I had previously made: and I am persuaded that although I may not have arrived at perfection, I have obtained some very interesting results; which in the hands of the experienced artizan, may become universally advantageous in this branch of employment.

The principles by which I have been guided in this application of the decomposing power of the electric current to the gilding metals, are the following:—

1st. The employment of feeble electric powers to effect decomposition when we wish to obtain a regular and uniform deposit of the particles of gold liberated from the solution, which is a chloride of that metal.

2nd. The employment of a bladder to keep separate the two liquids, placed one after the other, in the same electric circuit, and prevent their admixture whilst the electric current traversed them in succession. One of these is a solution of the muriate of gold, the other, water slightly acidulated, and in which the zinc piece is immersed.

3rd. The third principle is that which the electric current possesses, of passing with the *greatest* facility from a liquid to a metal, and *vice versa*, when the metal is *most* susceptible of being chemically attacked by the liquid. In the present case, the metal immersed in the solution of gold, is more easily attacked by the liquid than the gold itself; from which it follows, that the whole of the immersed part will not be completely gilt. The current, however, will seek the points where the metal is still bare, to traverse them and deposit gold on them, whatever may be the length of the route it has to pursue in the liquid, or however irregular and complicated may be the surface of the object intended to be gilt.

The following is the mode by which I have been enabled to apply the three preceding principles to gilding; the first two are due to

* This is the paper alluded to by the author in his paper on the same subject, given at p. 216 of this volume.—EDIT.

M. Becquerel ; and the third was announced, for the first time, in a memoir which I published in the *Annales de Chimie et de Physique*, for 1825.

I poured solution of chloride of gold, as neutral as can be obtained, and very much diluted, into a cylindrical bag made of bladder. I put this bag, with its contents, into a glass matrass, containing very slightly acidulated water. The object intended to be gilt communicates, by means of a thin wire, with a plate of zinc which is immersed in the acidulated water, whilst itself is immersed in the auriferous solution in the bladder. Or the metals and liquids may be reserved in the vessels, as occasion requires. In about one minute the object may be withdrawn and wiped with a clean, soft cloth, and it will be found already gilt. After two or three similar immersions, the layer of gold becomes so thick as to need no further continuation of the process.

The electric current must be very weak, and the disengagement of hydrogen on the surface of the object to be gilt should be avoided as much as possible, in order that deposition of the gold may not be interfered with. Consequently, only a very few drops of sulphuric, or of nitric acid, should be put into the water in which the zinc is immersed ; and this metal must not be sunk deeper than is just sufficient to establish the necessary current ; a little practice will enable the operator to manage this point very well.

The object to be gilt should be previously scoured and polished, or simply scoured only. By the first preparation a *brilliant* gilding will be obtained which appears to be owing to the polishing ; by the latter, the gilding is dull ; resembling that by the mercurial process when the objects are first taken from the fire. Perhaps the layers of gold are thicker, which I am led to suppose from the greater number of immersions required to effect the gilding. In all cases it is absolute necessary to scour the objects carefully, and clear their surfaces from grease. It is also advisable to wash them in acidulated water every time they are withdrawn from the gilding liquor ; and even after they have been rubbed, previously to the next immersion. A very good means of cleansing any article is to touch it with a piece of zinc for a few minutes, whilst immersed in acidulated water.

The colour of the gildings seems to depend on several circumstances :—on the quality of the dissolved gold, on the nature of the metal to be gilt, and on the degree of concentration of the auriferous liquor. The previous polishing of the surface, or its omission, seem also to affect the colour. When not polished beforehand, the gilding is much more red, an effect probably attributable to the gold being deposited on a comparatively rough surface. The mutual inclination of the asperities causes a play of light similar to that exhibited by the interior of a gilt vessel. It is very curious that burnishing does not destroy this effect.

It is necessary to be careful that the object to be gilt be not put

into the gold solution till everything is properly arranged, in order that the current may commence at that precise period; otherwise the *direct* action, of the liquid on the gold, would prevent the proper gilding from going on; and especially if the article were of silver.

This process appears to me very economical. Everything, excepting the gold, is quite cheap; and the gold itself is required only in small quantities to accomplish very beautiful gilding. I have gilt ten silver table spoons for less than threepence each. It is true the gilding was not very thick; it was of the fine greenish-yellow colour called English gold. It, however, resisted repeated frictions. A temperature of 300° or 400° did not alter it; but only made it penetrate farther into the metal. A single gilding over the first produced a very thick layer, which probably might be very durable.

By means of the above process, I have gilt wires, plates, silver coffee spoons, and brass watch-cases; and have succeeded on some watch-wheels. The extremities of the teeth were well gilt, but the colour was not that which watchmakers prefer; being too red. I am now attempting to discover some means of rendering it more yellow and also dull. It appears to me that every object, whatever may be its form, may easily be gilt by this process. A surface may also be partially gilt, either by covering with wax the parts which are not to receive the gilding, or by spreading the auriferous solution, with a pencil, on the parts to be gilt. Thus may be produced on a surface, by gilding, *traces* whose outlines form letters or figures.*

In conclusion, I may add, that since the completion of my labours, I have become acquainted with a process used in Germany and England, for gilding with a solution of oxide of gold in potassa. This process has not yet been extensively adopted; it requires a high temperature, whilst the electro-chemical one does not. It does not present the advantage, like the latter, of removing the oxygen and chlorine from the gold, and of preventing those bodies from attacking the articles to be gilt; an object which is obtained by the action of electric currents, by transporting them to the zinc of the voltaic arrangement. Moreover, the purely chemical process produces only a tarnished and dull gilding. I also consider it less economical; and it appears to me to present many more inconveniences than the electro-chemical process; of this, however, practical men will be the best judges. Be this as it may, experience will decide which of the two processes has the superiority. That which has encouraged me to make my process public, even should it have but a partial application, is, that I have observed that the merely chemical process seems to have been abandoned, and that the mercurial process continues in general use, which, as I have before mentioned, might be substituted by the electro-chemical process.

* For the first suggestion of covering the faces of medallions with gold and silver, either totally or partially, see "Annals of Electricity," &c.—Vol. IV, page 280.

MISCELLANEOUS.

Paris Academy of Sciences.—Feb. 1.

M. Ebelmen read a memoir on the nature of the various vapours developed in smelting furnaces, as observed at different altitudes within the furnaces. The object of such researches was to determine the degree of heat at various points, and to devise means for the improved regulation of the fires. He has arrived at the following results 1. The gaseous vapours, on coming out of a furnace heated by charcoal or wood, contain watery vapour, carbonic acid, and oxides of hydrogen and azote, but no carbonated hydrogen. At six or eight feet below the mouth of the furnace the watery vapour is not found, and the proportion of oxide of carbon increases, while those of hydrogen and carbonic acid diminish, according as the observations are made lower and lower down in the furnace. 2. When coal is used jointly with wood for heating the furnace, the carbonization of the vapours takes place in an internal zone, and the water is expelled from the metal at a very low altitude. He found that the proportion of gas which traverses a certain zone of the furnace per minute, is greater according as it is further from the bottom of the furnace. M. Arago gave an account of two memoirs, by Mr. Dove, "On the Phenomena of Chemical Induction." An electrical current causes, in a mass of iron placed near it, two kinds of phenomena—one corresponding to magnetism, the other to dynamical electricity. The author of these memoirs announced that he had succeeded in separating the two classes of action, by giving them different degrees of relative intensity; and he had shown the magnetic action to exist in substances where its presence had not been suspected.

DEATH OF THE GYMNOTUS.—The Electrical Eel at the Royal Adelaide Gallery died on the 14th instant. It was well known to all the visitors. It had been ill for a week, but it was not until Thursday last that there was any striking difference observable. It became very inactive, and this inactivity increased to torpor. The cause of its death was mortification. It was brought to this country from one of the many tributary streams of the river of the Amazons, about four years ago, and was the only one of its kind in Europe. Its structure was very singular. The seat of the electric power lay between the shoulder and the tail, and between the head and the shoulder. Its food was small fish, which it could stun and stupify by an electric shock, at two feet distance. It always stunned and stupified these fish before it ate them. The most interesting and beautiful experiment performed by its electricity was in setting fire to a piece of silver paper in a glass cylinder. One end of a conductor was attached to the paper and the other to the eel, and by this means the paper was burnt. It was necessary that the eel should be irritated before it would send forth electricity. It was young when brought over here, and was blind for some time before its death.

THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND
CHEMISTRY;
AND
GUARDIAN OF EXPERIMENTAL SCIENCE.

MAY, 1842.

MR. STURGEON'S RESEARCHES IN ELECTRICITY
AND MAGNETISM.

(Continued from page 231.)

IMPROVED ELECTRO-MAGNETIC APPARATUS.

*The Large Silver Medal and Thirty Guineas were this session presented to Mr. W. STURGEON, 8, Artillery-place, Woolwich, for his Improved Electro-Magnetic Apparatus. A set of which has been placed in the Society's Repository.**

MR. MARSH's apparatus, for the same purpose as Mr. Sturgeon's, was rewarded by the society in the session before last. The battery, consisting of plates presenting eight surfaces of about a square foot each, and weighing from twelve to thirteen pounds, was the smallest that had at that time been applied to electro-magnetic researches. The rest of his apparatus, with few exceptions, was such as had already been used by Ampère, Sir H. Davy, Mr. Barlow, Mr. Faraday, and others, fitted together in a very convenient way, and stowed in a box, which, with the battery, made a package sufficiently portable.

Mr. Sturgeon's apparatus is even more portable than Mr. Marsh's, and (the moving parts being at the same time larger) is better fitted than that for the use of the lecturer.

The battery is similar in construction to Professor Hare's calorimotor, and consists of two fixed, hollow, concentric cylinders of thin copper, having a moveable cylinder of zinc placed between them. Its superficial area is only 130 square inches, and it weighs no more than 1lb. 5oz. It is moveable on an upright metallic rod, like a laboratory lamp, and may therefore be adjusted to any convenient height. It will not hold more than about a pint of

* Extracted from the Transactions of the Society of Arts, for 1825.
Ann. of Elec. Vol. VIII, No. 47. May, 1842.

liquid, which should be composed of one part nitric acid, and eleven parts water: it is charged with the greatest ease, by merely pouring in the liquor from a common lipped jug. A farther advantage of the construction is, that after every experiment the liquor may be returned to the jug, while preparations are making for the next, by which the battery, when wanted, is in a state of high activity, and is undergoing no deterioration in the interval between one experiment and another.

A farther point of novelty is, that Mr. Sturgeon has very judiciously chosen to have a small galvanic power, assisted by a strong magnetic power, rather than the reverse, as is usually the case. This has enabled him greatly to economise in the size of his battery, and in the cost of acid to excite it, while the increased magnetic power is obtained at a small first expense, and needs no renewing.

Letter to the Secretary of the Society of Arts.

SIR,

No, 9, Artillery-place, Woolwich.

The science of electro-magnetism, although so generally interesting, yet (comparatively speaking) appears to be very little understood. This latter circumstance is probably, in a great measure, owing to the difficulty of making the experiments, and the great expense attending the process; for, besides the first price of a large battery, considerable expense in acid must always attend its excitation, whenever an experiment is attempted. Large batteries are always attended with difficulty of management, and the great quantity of hydrogen evolved during the process renders the use of them extremely inconvenient to the operator. These are evidently great obstacles to the experiments being often repeated, and to the science being generally known. Another, and perhaps no less obstacle to the advancement of this interesting science, is, that the experiments being hitherto exhibited on so small a scale, are by no means calculated to illustrate the subject in public lectures; for when the experimenter succeeds even to his wishes (which is not frequently the case), the experiment can only be seen by a very near observer, and the more distant part of the auditory are obliged to take for granted what they hear reported (from those persons who are more favourably situated), of some of the most interesting facts, which they, from their distance, are unable to witness.

With a view of removing, in some measure, these apparently formidable obstacles in the progress of this infant science, I have devoted a considerable portion of time, labour, and expense, in repeating several of the experiments, under various circumstances, and with various forms and sizes of batteries. I have likewise instituted a series of experiments, for the purpose of discovering, if possible, if any particular ratio of galvanic and magnetic power was

absolutely necessary to be observed in the process of electro-magnetism. If no particular proportion of those two powers was essential, then it appeared highly probable that an increase of magnetic power might compensate for a deficiency of the galvanic, and thereby render the use of large galvanic batteries quite unnecessary, an object which I considered both interesting in its nature, and, by reducing the expense, and facilitating the process, exceedingly desirable to the experimenter; and I am happy to state, that my labours were no ways abortive, for instead of electro-magnetic phenomena depending on powerful galvanic, and feeble magnetic force, as had till then been practised, I found, during that inquiry, that the galvanic force may be reduced to almost any degree, provided the magnetic be sufficiently powerful. This discovery led me to the use of powerful magnets, and small galvanic batteries, for with small magnets the experiments can never be made on a large scale, although the galvanic force be ever so powerful; and as minute and delicate experiments are not calculated for sufficiently conspicuous illustration in public lectures, I considered that an apparatus for exhibiting the experiments on a large scale, and with easy management, would not only be well adapted to the lecture room, but absolutely valuable to the advancement of the science. Upon this principle I have constructed a complete set of instruments, which, from their superior magnitude, and peculiar arrangement, in my humble opinion, and by the certificates I have been honoured with, are, in the opinion of gentlemen whose judgment I presume will ever be held in the highest estimation, well adapted for the illustration of the subject, either in the private study or public lecture room.

It will be understood from what I have already stated, as well as from an inspection of the instruments, that the mode which I have taken for the production of electro-magnetic phenomena is more simple in its management, less expensive in the process, better calculated for the illustration of the subject, and the reverse of that which has hitherto been used, and which, by its almost entire dependence on the tedious and expensive process of galvanism, has considerably retarded and obscured this new and interesting science; for whenever an experiment was not attended with the anticipated success, the failure was generally attributed to an insufficiency of galvanic power; and in order to increase the effect, it appears that the experimenter had no other means of accomplishing his object, than by augmenting the power of his battery, or by reducing the size and increasing the delicacy of his other apparatus, the magnetic power being either entirely lost sight of, or regardlessly neglected, as if no ways materially concerned in the process.

I have found, however, by the above-mentioned course of experiments, that the magnetic force is as essential as that of galvanism to the development of electro-magnetic phenomena; and the apparatus which I now submit to the attention and impartial consideration of

your valuable society, acting on the principle of powerful magnetism and feeble galvanism, will, I trust, be found more eligible and efficient than any other that has yet been brought before the public.

A. Aikin, Esq.

Secretary, &c., &c.

I am, Sir, &c., &c., &c.,

W. STURGEON.

CERTIFICATES.

Royal Military Academy, Woolwich Common,
9th May, 1825.

SIR,

As I understand that Mr. William Sturgeon proposes to submit his galvanic battery and electro-magnetic apparatus to the consideration of the Society for the Encouragement of Arts, &c., I take the liberty of recommending them to your favourable consideration, although I have not the honour of being personally known to you. Since the first discovery of the action of the galvanic circuit on the magnetic needle, I have witnessed all the principal phenomena connected with electro-magnetism as exhibited by batteries and apparatuses of different constructions, and have no hesitation in saying that the battery and apparatus of Mr. Sturgeon are most admirably calculated for exhibiting all these phenomena in the most striking manner, and that they afford peculiar facilities for investigation in this most interesting science. The expense attending the purchase and use of large galvanic batteries, the difficulty of keeping them in powerful action, and the inconvenience of using them, except in laboratories, must all tend to retard the progress of a science, which has opened a new field for inquiry; and as the battery and apparatus of Mr. Sturgeon are free from all these objections, I consider that, by rendering them public, the cause of science is promoted, and that he is deserving of every encouragement for their invention and successful application.

I am, Sir,

A. Aikin, Esq.,

Secretary, &c., &c.

&c., &c., &c.

S. H. CHRISTIE, M.A.

Royal Military Academy, Woolwich,
May 9th, 1825.

SIR,

I beg leave to introduce to the favourable notice of the society, Mr. William Sturgeon, a resident of this town, who has for some time devoted himself most sedulously to the improvement of apparatus for the development and exhibition of the properties of electro-magnetism. He wishes to solicit the patronage of your valuable institution to a new *battery* of his invention, which, while it is very far smaller than any other of which I have heard, is very efficacious, and at the same time remarkably calculated to facilitate the experiments. Mr. Sturgeon has also, in my judgment, greatly improved several other classes of apparatus employed in this interesting de-

partment of research, and constructed them upon a scale which renders the experiments susceptible of pleasing, and successful exhibition to a large auditory. He will regard himself as highly honoured, by permission to exhibit them before a committee of the society; and I cannot but believe, that if he be so honoured, his talents and ingenuity, tending, as they have done, to most curious and pleasing results, will entitle him to favourable consideration and an appropriate reward.

I am, Sir,

&c., &c., &c.,

A. Aikin, Esq.,
Secretary, &c., &c.

OLINTHUS GREGORY,
Professor of Mathematics.

Having read the above certificate of Dr. Gregory, and that of Mr. Christie, I have no hesitation in adding my testimonial to every thing there stated.

PETER BARLOW.

Reference to the Engraving of Mr. W. STURGEON'S Electro-Magnetic Apparatus.—Plates III and IV.

Plate III, Fig. 1. A perspective view of an apparatus to show the revolution of a magnet round its own axis. *a a*, the two galvanic apparatuses on their stands *b b*, they are acting on the magnet *n s*, by means of the connecting wires *d d d d*; both their copper poles *c c*, are applied to the equator *e*, of the magnet; while the zinc pole *z*, of one is applied to the north pole *n*, and the zinc pole *z*, of the other is applied to the south pole *s*, of the magnet. A wire *f*, is soldered on to the magnet, and bent down at one end to dip into the circular trough *e*, to form the equatorial connection: and as all the connections are made by mercury and amalgamated wires, the end of this wire is amalgamated, and mercury put into the trough: all the little cups *z* and *c*, are also amalgamated at the bottom, and contain mercury; the bottom wires of the zinc and copper poles, are likewise amalgamated to dip in connecting cups when wanted. The magnet has brass wire centers on which it turns; that at the north pole stands in a cup at *z*, with mercury; and the other at the south pole, enters the amalgamated hollow in the screwed end of the upper connecting cup *z*. When the connections are made, as above described, on pouring in dilute nitric acid into the troughs *a a*, the magnet will revolve in the way shown by the arrow; but on changing the connections, by applying the copper wires to the poles, and the zinc ones to the equator, it will revolve the contrary way; here the magnet only forms the connection between the electric poles, and revolves around, or with the current which is conducted by it. *g g g*, is the stand which supports the magnet; the

equatorial trough *e*, is made moveable on the pillars *g g*, and is fixed by the screws *h h*.

Fig. 2. A bird's-eye view of the same, without the stands *b b* and *g*. The galvanic troughs *a a* are copper and cylindrical, having a smaller split cylinder of copper, soldered within to increase the copper surface; the intermediate cylinder in each trough is zinc, it has three cork feet, cemented to the bottom edge to prevent contact with the copper, and two pieces of cork *i i* cemented at top for the same purpose; *e* the annular trough.

Fig 3 shows the magnet separate.

Fig. 4. A wooden cover; one is fitted to each trough *a a* to prevent the ebullition of the acid from damaging the apparatus.

Figs. 5 and 6. A side and front view of a circular metal disk, made to revolve between the poles of a horse-shoe magnet; the disk is amalgamated round its edge, and dips into a little mercury contained in a hollow *j* of the stand, the centres *k k* on which it turns, and the hollows that receive them in the forked support *l l*, are amalgamated; the screw *m* allows the disk to be adjusted, and fixed so as only just to touch the surface of the mercury. A horse-shoe magnet *n* or *n s* shown by dotted lines, is laid on the stand, then one of the troughs *a* of fig 1, is to be adjusted on its stand *b*, till its bottom wire *z* dips into the connecting cup *z*, forming the zinc communication, and a connecting wire *d* with bent ends, is to dip into the copper connecting cup *c* of the trough, and into the cup *c* of the disk; the communication of the poles being thus made, (the current passes from *z*, through the mercury *j*, into the edge of the disk, and through its centres *k k* into the fork *l l*, and up to the cup *c*,) the disk will then revolve as shown by the arrow. By reversing either the poles of the magnet, or the electric poles, the revolution of the wheel is reversed; but if both are reversed, the revolution will continue in the same way as at first. The six rays are painted on the disk, merely to render the revolution visible at a greater distance.

Fig. 7. A stand supporting a needle between two conducting wires *o o*, and *p p*, to show the different effect of electricity on the needle when passing above or below it; the cup *z*, is common to both, but the other ends have each a separate cup *c c*: when the electric current passes along the upper wire *p p*, the needle takes the position as shown in fig. 8; but on lifting the connecting wire out of the cup *p c*, and putting it into the cup *o c*, the current passes through under the wire *o o*, and the needle immediately goes round to the position indicated in fig. 9; then if you watch the motion of the needle, and keep alternately transferring the wire out of one cup into the other, keeping time with the needle, you may bring it into the most rapid revolution that you can possibly keep time with.

Plate IV, figs. 19 and 20, show a side and front view of a dipping needle, mounted between two wires *o* and *p*; they are here placed in the direction of the dip, but the quadrant *i*, allows them to move one quarter round, or to the equator of the magnet, as shown by

dotted lines. In their present position the needle will deviate, as figs. 8 and 9, plate III; and it will be seen the needle cannot take a position quite at right angles to the wire, owing to the terrestrial magnetism drawing it on one side; but when the wires are carried round to the dotted position, fig. 19, plate IV, the needle remaining as it was, so as to be at right angles to each other, then on passing the current from *z* through the wire *o o*, no effect will appear to take place, the needle is only more confirmed to its position; but on passing it through *p p*, the needle goes round, and dips with its south pole. The wire passes through the wooden cup *z*, but the two ends of it *p* and *o* only just enter their respective wooden cups *c c*; these wooden cups are placed at an angle of 45° to the horizon, so that in either position they are similar, and will hold mercury enough to make the contact.

Plate III, fig 10, shows an arrangement for making a cylinder revolve, by applying a battery of magnets to the outside. The cylinder and stand are in section to show the support.

Fig. 11 is a bird's-eye view of the same apparatus, the cylinder being removed, and its place shown by a dotted circle; the middle portion *q* contains mercury, which also flows in a very narrow arm to the side where the magnets are placed. The sides of this narrow arm are so low that the convex surface of the mercury rises above them, and the edge of the cylinder, which is amalgamated, passes over, as shown by the dotted circle, and keeps in contact with the convex surface of the mercury without touching the sides of this little trough. The cylinder, fig. 10, has a sharp point in its crown, by which it hangs freely on the top of the central wire *r*; this wire fits into a pipe or hole *s* in the stand, and the mercurial trough *q* being varnished, prevents this axis from forming a communication. There is also a little connecting cup *t*, for mercury on the top of the cylinder, into which the screw of the upper connecting cup *c* dips just enough to touch the mercury; *v v* is a wooden stand holding six magnets, the north poles of which are placed near the cylinder on the side that dips into the mercury; then on making the galvanic communication with *z* and *c*, the current passes through the wire of *z* into the mercury *q*, and out of the mercury up that side of the cylinder opposed to the magnets, and meets the copper pole *c* at top. Now the magnets are continually propelling that portion of the cylinder which forms the connection, and as the succeeding portions continue to form the connection with the mercury, only opposite to where the magnets are placed, they are as continually propelled, and thus a revolution is produced; the arrows show the direction of the motion, and changing the poles changes the direction.

Fig. 12, *w w*, shows the arm which supports the upper conducting cup, it fits into the pipe *x*, which is screwed to the foot.

Figs. 13 and 14. A front and side view of a stand with two connecting cups *z* and *c*, made of wood, in which the bent wire wound round with copper wire is supported by the two copper wire ends.

On making the galvanic connection through the copper wire, the iron wire becomes a strong horse-shoe magnet, and will support a heavy bar of iron as *y*, fig. 14; but on lifting the connecting wire *d*, fig. 13, out of the cup *z*, the weight immediately drops, and on restoring the connection, the power is restored; then if you change *z* for *c*, it will change *n* for *s*, or if you only wrap the copper wire about the iron wire, as a right threaded screw instead of a left one, as in the drawing, it will change *n* for *s*. This is explained by what takes place in figs. 7, 8, and 9.

Plate IV, fig. 1. Another copper wire fitting the stand.

Plate III, fig. 13. This arrangement communicates magnetism to hardened steel bars, as soon as they are put in, and renders iron wire within it magnetic during the time of action; it only differs from fig. 13 in being straight, and thereby allows the steel or iron bars to slide in or out.

Plate IV, fig. 2, shows the revolution of two magnets round two similar electrified wires *g g*; a stand supported by three pillars *h h h*; *e e* two annular wooden troughs, one is shown separately in section fig. 3; the neck *i* fits easily into corresponding holes in the stand *g g*: *j j* two standards with pointed tops; *n s*, *s n*, the two magnets are bent as fig. 4, and have hollows in the middle, by which they swing on the standard points *j j*: fine copper wires *k k*, are twisted tight on each magnet, and go loosely round the standards *j j*: they serve to keep the magnets upright; *l* a brass standard fixed at the back of the stand, and bent forwards at top, where it is split to receive the flat piece *m*, which is secured by the screw *o*; this flat piece carries the bent wire *p p*, having cups *c c* on its shoulders, and points at bottom, to enter without touching the cups *q q*; *f f*, figs. 4 and 5, are bent wires, cemented on the middle of the magnets; these pass through the cups *q q*, and dip into the troughs *e e*, fig. 2; into these as well as into the cups *c c*, and *z z*, mercury is placed to form the connection, then bringing the galvanic troughs *a a*, and dipping the zinc poles into *z z*, at the equators of each magnet, and uniting the copper poles *c c*, by the wires *d d*, to the upper cups *c c*, the circuits are completed, and the magnets will revolve, as shown by the arrows, around the wires *p p*.

Fig. 6 a horse-shoe magnet, mounted with two mercurial troughs *r r* (fig. 7 shows one separate); *t t*, two cylinders suspended on the ends of the magnets, by points within their crowns under the cups *v v*; their bottom edges are filed away, leaving only four points (as fig. 8) to touch the mercury, by which means the friction is much lessened. The troughs are adjusted by the screws *u u*, so as to bring the mercury just in contact with the points of the cylinder; the screw points of the upper cups *c c*, just touch the mercury in the cups *v*. Upon making the communications as before with the cups *z z*, and *c c*, the cylinder will revolve as shown by the arrows.

Fig. 8, *w* is one of the wires which holds the upper cups *c c*; the bent end *w*, twists into a hole in the side of the trough *r*, and thus

supports the wire when it is necessary to turn it over on one side previous to removing the cylinders.

Fig. 9, shows a stand on which a rectangular wire is suspended on one of its sharp pointed and amalgamated ends, which is here the copper end; the other or zinc end, although tied to it, is kept separate by the silk thread passing between them; this zinc end dips into the little mercurial cup *x*, from which the small wire *z*, descends to the cup *z* at bottom; the cup *c*, is united to the standard (the cup *x*, and the part of the standard that rises through it is well varnished to insulate the mercury from the standard); on making the zinc and copper communication with these cups, the circuit is completed through the rectangular wire, and it will then take the position east and west; but in changing the communication the west side will go round to the east.

Fig. 10, shows another rectangular wire through which the circuit is completed; the side *y*, when offered to the west side of fig. 9, repels it, the current being in contrary directions through them, as shown by the arrows, but it will attract the east side, the two currents being then alike.

Fig. 11 is a compound or double horse-shoe magnet, fixed upright in a block of wood *a a*. Two brass tubes shown in section *b b*, are fixed in the board *c c*, and fit on over the magnet, making them appear as two pillars; they are secured to the bottom block *a*, by two screws *d*. On the top of these are fitted the two circular mercurial troughs *e e* (they are shown separate in figs. 12 and 13). The upper cups *c c* have loops, or eyes, at the bottom of their screws, which are amalgamated as well as the hooks of the wires on which the pith balls *f f* are placed. The lower ends of the wires dip into the mercury in the troughs *e e*: on making the communication, or circuit through *z c*, *z c*, the wires will revolve as shown by the arrows. The upper cup *c* is insulated from the arm *w*, fig. 13, by the piece *u* being of hard wood.

Fig. 14 shows one of a pair of troughs fitted to the same magnet, it has a fixed arm or arch *g*, through the top of which passes a cup-headed screw, also a smaller arm with the stud *h*, to hang on the top of the pillar *b*, fig. 11. The trough is varnished, to enable it to hold dilute acid. On the stud *h* hangs the zinc cylinder shown separate fig. 15. It is adjusted by bringing the screw in the arch *g* to touch the mercury in the little cup, then the copper cylinder, fig. 16, is hung on the screw-head by its amalgamated point, completing the circuit through the screw of the fixed arch *g*; on filling these vessels with dilute acid the zinc and copper cylinders will revolve as shown in fig. 17.

Fig. 18 shows two of the connecting wires separate, three or four pairs of each of these are required.

These figures are nearly one-fifth of the real size, and it will be seen that the magnetic power is very great in proportion to the galvanic power.

*Memoir on Induction. By M. M. MASSON and BREGUET.**(Extracted by the Authors.)**

When a very long wire is traversed by a voltaic current, the points situated at equal distances from the extremities of this wire, appear to us to be charged with static electricity of contrary signs, capable of charging a condensing electroscope.

At the moment of closing and breaking the current, these tensions seem to augment and to acquire a great vigour by twisting the wires into helices.

When the wire is wound into an helix, the tension is so much augmented at the point of interruption of the circuit, that we may easily obtain sparks of two centimetres long in a vacuum.

These phenomena of induction appear to be due to actions exercised at a distance by the static electricities on the neighbouring wires, and are also presented in the phenomena of electric influence obtained by the electrical machine.

The electric light obtained in vacuo by the extra currents, or the currents of induction, present the same character as that which is obtained in the same circumstances with electrical machines or the Leyden jar.

When two helices are placed one on the other, the one receiving the current of the pile, we experience shocks on taking hold of one extremity of the extra current, and one extremity of the primary helix ; if another person takes the two other extremities which were left at liberty, the shocks will be increased.

Three helices being placed one on the other, if the extremities of the middle one are joined, the interrupted current of the pile passing into the first, will not induce one in the third ; but if we make the ends of the middle helix communicate with a very long wire, it then no longer forms a screen, and the shocks are felt in the third.

When by the disposition and the length of a wire helically wound, we obtain by the extra current, or the current of induction, the electric spark in a vacuum, this light will disappear immediately on putting a cylinder of soft iron into the helix, and will re-appear on withdrawing it.

The static and dynamic states of electricity, are two modes which are susceptible of being transformed one into the other, and by the words *intensity* and *quantity*, we must understand equal quantities of active electric forces, which only differ as to the duration of their action.

Thoughts on Magnetism, by RICHARD KIRWAN, Esq., L.L.D.,
F.R.S., and M.R.I.A.†

1. There are two ways of explaining a natural phenomenon ; the first, is by discovering the conditions and circumstances of its pro-

* Comptes Rendus, 23rd August, 1841.

† Transactions of the Royal Irish Academy. Vol. VI.

duction and the laws by which its action is governed ; the second, is by shewing its analogy, similarity, or coincidence with some general fact with whose laws or existence we are already acquainted ; this last mode is by far the most perfect and satisfactory. In the first sense of the word, electricity and magnetism have been in some measure explained, but in the last sense neither ; the primary cause of magnetism in particular has hitherto been supposed to relate to iron alone, or its ores, and to stand unconnected with all other natural phenomena.

2. If therefore any other general fact or power can be discovered to which it bears some analogy or similarity, it may so far be said to be explained. Now such fact or power I think may be assigned, namely, the power of *crystalization*.

3. By crystalization I understand that power by which the integrant particles of any solid possessing sufficient liberty of motion unite to each other, not indiscriminately and confusedly, but according to a peculiar uniform arrangement, so as to exhibit in its last and most perfect stage regular and determinate forms.

4. This power is now known to be possessed by all solid mineral substances.

5. The forms which crystals, even of homogeneous substances, exhibit, are often very numerous ; however in most cases they may be reduced to a few primordial forms, which, as Abbé Hauy has lately experimentally proved, are derived from certain original forms appertaining to the minutest particles of their concretion.

6. The assemblage of these ultimate particles into visible aggregates, similarly arranged, necessarily requires that one of their surfaces should be attractive of that particular surface of the other, which presents a corresponding angle, and repulsive of that which presents a different angle, otherwise the various regular rhomboidal and other polygon prisms and pyramids, which crystals present to us, could never exist ; consequently the minutest prism, being once formed, could never be prolonged if one end of such prisms were not attractive, and the other repulsive of the same given surface.

7. Hence it has been observed that crystalization never takes place in the middle of any solution, but always begins at the surface or on the bottom or sides of the vessels that contain it, for the particles in the middle of the solution being confusedly mixed with each other, and exerting their repulsive as freely as their attractive powers, the one constantly counteracting the other, no sensible accretion of a regular kind could take place, whereas the repulsive power of the uppermost particles, or of those that rest on the sides or bottom of the vessel, is restrained and impeded.

8. The repulsive power of crystallizing substances also appears in many other instances (of the attractive no doubt has ever been formed). Thus if saturate solutions of nitre, common salt, and tartar vitriolate be mixed and set to crystallize, each will crystallize a part, which could not happen if the particles of each of these salts

did not only attract their similar homogeneous, but also repel those of a different species, otherwise the mere casual circumstance of greater proximity to one than to the other would impel them to unite indiscriminately. Again, if a saturate solution of allum be mixed with a turbid mixture of clay, and abandoned to insensible evaporation, after some time the clay will subside and form a dry mass, but in the interior of this mass large regular crystals of allum will be found, whose component particles must, to reunite, have displaced and repelled the particles of clay with which they were surrounded.

9. If to a saturate solution of a salt that difficultly crystalizes, a crystal of a salt of the same species be inserted, the whole solution will soon be brought to crystalize, as the crystal inserted attracts the particles dissolved, by its different surfaces; but if a salt of a different nature be inserted this will not happen, crystalization will not be promoted.

10. If to a solution of 2 parts nitre, and 3 parts Glauber's salt in 5 parts water, a crystal of nitre be inserted, the nitre alone will crystallize; or if instead of nitre a crystal of Glauber be inserted into it, the Glauber alone will crystalize. Do not these experiments fully evince both the attractive and repulsive powers, not only of different salts, but of different surfaces of the same salt?

11. These powers within their proper sphere of action have been found indefinitely great; thus water confined in cannon several inches thick, and exposed to a degree of cold much beneath the freezing point, has been observed to crystalize into ice that burst the metallic impediment opposed to the form it then assumes.

12. The vast difference however attending the development of these two powers (of magnetism and crystalization) will undoubtedly strike many as an insuperable objection to their identity, yet their *direction* in all its varieties being exactly the same, difference in other circumstances seems to me to indicate rather a variety of *degrees* in the same power, than any essential difference in the powers themselves.

I now come to the application of the above principles to the magnetic phenomena. These may in general be reduced to the following, viz. *Attraction, Repulsion, Polarity.*

Communication.

Declination.

Inclination.

Exclusive appropriation to Iron.

Destruction of the Magnetic Power.

1st, *Attraction, Repulsion, Polarity.*

The quantity of iron found on and within such parts of the surface of the globe as we are acquainted with, far surpasses that of any other mineral substance singly taken, or even of many of them taken together; scarce any stone or metallic ore, or earth, is found free

from it ; it enters into their composition in the proportion of from two to eighteen or twenty per cent., and perhaps at a medium we may state it in all of them at six per cent. ; moreover its own ores are of all others, the most common and the most copious ; in many places, particularly in the most northern climates, whole mountains of it are found, and many of them magnetic. When to this consideration we add that of the specific gravity of the globe, which has been found to be 4.5 times heavier than water, notwithstanding the immense quantity of water that covers the greater part of its surface to considerable unknown depths, and notwithstanding that the specific gravity of by far the greater part of the stones and earths it contains, does not exceed, and scarcely amounts even to three times the weight of an equal bulk of water, and that the quantity of mineral substances whose specific weight exceeds four times that of water, is almost infinitely small in comparison to the other known component parts of the globe, and finally, that the weight of most iron ores, is about four or five times that of water ; all this I say considered, it is difficult to avoid concluding that the interior part of the globe consists chiefly of iron ore, disposed in one or more aggregate masses ; a conclusion that is farther confirmed, on reflecting that volcanic lavas ejected from the deepest recesses with which we are acquainted, contain from fifteen to twenty or twenty-five per cent. of iron in the state most favourable to magnetic attraction.

Taking then this assertion to be as fully proved as its subject matter is capable of being ascertained, we may deduce from it the following corollaries :

1st. That as the ferruginous matter in the globe being by far the most copious, its universal attractive power is principally seated in the ferruginous part.

2nd. That as all terraqueous matter was originally in a soft state, its parts were at liberty to arrange themselves according to the laws of their mutual attraction, and in fact did coalesce and crystallize in the direction in which they were least impeded by the rotatory motion of the globe, namely, in that which extends from north to south, and principally and most perfectly in the parts least agitated by that motion, namely, those next the centre.

3rd. That this crystallization, like that of salts, might have taken place in one or more separate *shoots*, or as we may here call them, immense separate masses, each having its poles distinct from those of the other, those in the same direction repulsive of and distant from each other.

In consequence then of the universal law of attraction of the particles of matter to each other, these internal magnets exert a *double* power of attraction ; the first and most general, on the particles of *all* bodies indiscriminately in proportion to their density, and the direct or inverse ratio of the squares of their distances, according as those bodies are found within or without the earth's surface ; and the second, on bodies of their own species in proportion to their

homogeneity, and to the correspondence of the arrangement of their integrant particles with that of the integrant particles of these internal magnets.

A magnet, therefore, is a mass of iron, or of iron ore, whose oxygenation does not exceed 20 per cent., or thereabouts, whose particles are arranged in a direction similar to that of the great internal central magnets of the globe. This I call the *magnetic arrangement*.

The particles of iron attract each other more forcibly than those of any other known substance. This appears by its cohesion, hardness, elasticity, and infusibility, in each of which properties, or at least in the combination of most of them, it exceeds all other known bodies.

Hence, a magnet *attracts iron* when within the sphere of its action, by forcing, in virtue of its attractive power, a certain proportion of its integrant particles into a disposition and arrangement similar to that of its own. For in this case it exerts a double attractive power, that of the particles of iron to each other, which we have seen to be the greatest of all others, and that of crystallizing bodies, which we have also seen to be indefinitely great.

The crystallizing power being at once attractive and *repulsive*, according to the direction of the surfaces (No. 6), hence we see that one part or end of the magnet must repel that which the other has attracted, as long as the same disposition of parts remains.

The disposition of parts in a particular magnet, being similar to that which obtains in the great internal general magnet, extends in the direction of from north to south. Hence magnets, when at liberty to move with a certain degree of freedom, and iron, when a sufficient number of its particles are arranged in that direction, and has sufficient liberty to conform to it, points to those poles. Hence this property is called *Polarity*.

The magnetic power is greater or lesser according to the number and homogeneity of the particles *similarly* and *magnetically* arranged. Hence small magnets may be more powerful than a larger, and hence a magnet will attract a magnetized needle at a greater distance than one not magnetized.

The magnetic power decreases in a certain ratio of the distance of the particles that exercise it. Hence it is strongest at the point of contact, and at the poles, as it is there most *unsaturated*, and weakest in the central part, which separates the two opposite poles.

When a magnet is broken into small pieces its power is nearly destroyed, because, though the poles should be all of the same kind, yet the distance of each other from the opposite pole is so small that their powers counteract, and consequently destroy each other.

If, when a needle is attracted by the south pole of a magnet, a bar of iron be placed on the north pole, the needle is still more strongly attracted, because the iron acquires also a south pole, whose force is joined to that of the magnet.

If two needles be suspended from any given pole of a magnet, they will diverge, because they both require the same polar arrangement. If a bar of iron be laid on that pole of the magnet, the divergence will diminish, because the next end of the iron will require the disposition of the opposite pole, and consequently, counteract the repulsive power of the magnet.

A magnet will not transmit its power through a bar of iron, if this be too long. Muschenbrouck limits their length to six feet, but this depends on the strength of the magnet.

The power of a magnet (every thing else being equal) depends on the *numbers of its surfaces magnetically arranged*, and the *accuracy* of that arrangement.

The arrangement is accurate when the synonymous surfaces are exactly parallel to each other, and originally conformed to and parallel with those of the great general magnet.

The magnetic attraction is strongest in the direction perpendicular to the magnetic surfaces, and weakens in proportion to the magnitude of the angle of direction with the perpendicular, and consequently, is *null* when at a right angle with it. Hence the magnetic power seems concentrated at the *poles*, and the lateral powers are the weakest, as they originate only in the oblique direction of surfaces, or from surfaces inaccurately arranged.

If the south pole of a magnet be charged with filings of iron, and then approached to a bar of iron standing erect, part of the filings will drop off, because the poles of the same name, by exciting a contrary arrangement, weaken each other; but if the filings were suspended from the north pole of the magnet, it would take up still more of the filings, as the opposite poles strengthen each other; the uppermost pole of the iron in this case, becoming magnetic by position.

If the synonymous poles of two magnets of equal powers be approached to each other, if the powers be very unequal, the stronger immediately destroys the weaker, and inducing a contrary disposition attracts instead of repelling it; if the powers be less unequal, it requires a longer time; so also, if one be *softer* than the other. Even if their powers be equal, yet, after some time, the softer will yield to the harder. If both be equally hard, they only *weaken* each other.

If a magnet be cut in two, in a direction parallel to the axis, the parts before conjoined will now repel each other, because they still retain two synonymous poles.

But if the magnet be cut in two in a direction perpendicular to the axis, the two ends before conjoined will now attract each other.

If a magnetic wire be twisted, its powers are so disordered that one side of the wire, in some places of it, will be attracted and the other side repelled by the same pole of the magnet.

The power of the magnets (*cæteris parib.*) is in proportion to their surfaces, or as the squares of their diameters.—See *Hutton's Magnetism*, p. 72.

Communication.

When iron is applied to or brought within the sphere of activity of a particular magnet, it acquires the arrangement requisite to form the heteronymous pole, and thus becomes itself in some degree magnetic in its whole length, if this length be not totally disproportioned to the power of the particular magnet.

Hence the other end of such bar of iron acquires the arrangement of the opposite pole, according to the laws of crystallization already laid down.

Iron becomes magnetic either by contact or proximity to a magnet, or by position, or by internal commotion.

If a bar of iron be placed in a vertical position, its insensible fibrillæ gradually acquire the magnetic arrangement, so that after some years it becomes a complete magnet, its *lowest* part becoming a north pole, that is, pointing, when free, to the north, and the *upper* a south pole. In the south hemisphere the under end becomes a south pole.

A bar of iron not previously magnetic, does not acquire this disposition in the slightest degree while lying in a horizontal or nearly horizontal disposition, but if one end of it be raised, it immediately acquires it in some degree, as appears by approaching a magnetic needle to either end, because in that direction it is then exposed to activity of the polar ends of the great general magnet.

But if a bar of iron be heated, though only at one end, and while hot set in a vertical or nearly a vertical position, it will acquire the magnetic power much more readily.

So also if one end of a bar of iron not magnetic be struck against the ground, it will become in some degree magnetic, the lower end becoming a north pole, &c.; and if afterwards the other end be struck in the same manner, the poles will be reversed.

Hence it is evident that *any motion* communicated to the integrant particles of iron, placed in a proper situation, helps them to assume the magnetic disposition already impressed upon them by the great general magnet.

If the opposite poles of two magnets of *equal power* be approached to each other, the power of both is increased; and if one of them be *more powerful* than the other, it will increase the magnetic disposition, and consequently the power of the weaker.

Soft iron, as its parts are most easily moved, receives the magnetic disposition most easily; *hard* iron or tempered steel more difficultly, and *cast* iron, as being both hard and abounding in the heterogeneous particles, most difficultly and imperfectly.

Whatever way iron is applied to a magnet the magnetic power is diffused in the direction of its length. Hence it should seem that when a bar of iron is laid on a magnet, the contiguous ends of the iron become poles of the *same name* with those of the magnet to which they are contiguous, and hence may be derived the power of

armed magnets, for the surfaces of the *armour* immediately beneath those of the magnet impress a direction opposite to their own on those of the magnet, and consequently rectify such surfaces of the magnet as may have been inaccurately directed, and thus strengthen it.

To communicate the magnetic power to iron by friction against a magnet, it is necessary that its pole should slide along the magnet several times in the same direction, for if the directions be alternately opposed, the powers received will successively destroy each other.

A synonymous pole is formed at the end at which the friction begins to that of the magnet applied, and an opposite at that at which it terminates,

Appropriation to Iron.

It has of old been observed that the magnetic phenomena were peculiar to iron, and the reasons why they are so have been already assigned, but of late some semi-metals have been observed to partake of these properties, as nickel, cobalt and manganese; this has been thought to arise from a mixture of ferruginous particles, from which they can be scarcely freed, and with respect to manganese, and in many cases of the others also, this seems to hold true; but with respect to nickel, and in some instances of the others also, the magnetic properties they discover seem to me to proceed from their great attraction to iron, particularly when their particles are duly arranged, for then they are exposed to the power of the great general magnet, which acts on them in proportion to this arrangement and their affinity to iron.

Of Inclination and Declination.

These phenomena, which are so different in different parts of the globe, and even in different seasons and hours of the day, not being as yet noted with sufficient certainty and precision, I shall, for the present, decline entering into their explanation.

On the Electricity produced by Evaporation.

(Continued from page 186.)

IN the first paragraph of page 186, we have stated that "the curiosity of philosophers concerning Volta's experiments having become subsided, we hear little more of the production of electricity by evaporation till the celebrated experiments of Messrs. Armstrong and Pattison." Since the publication of that *historical sketch*, we have been reminded of the following series of experiments, on this subject, by the Rev. Abraham Bennet, prior to the year 1782.

"In a treatise on electricity by M. L'Abbe Haüy," says Mr. Bennet, "I find that since M. Volta's discovery of electricity produced by

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evaporation of water from hot coals, Messrs. Lavoisier and De la Place have remarked, that bodies passing from a solid or fluid state into vapour, give unequivocal signs of positive or negative electricity. A large vessel containing a quantity of iron filings was insulated and connected with M. Volta's condenser. Three parts of water and one of vitriolic acid were poured upon the filings, which caused a brisk effervescence, and a rapid discharge of inflammable air, and in a few minutes the condenser became so strongly charged that it gave a very sensible spark, and by the electrometer it was found to be negative. The production of fixed and nitrous airs had the same effect; also chafing dishes insulated and filled with lighted coal produced very clear signs of negative electricity after the combustion of the coal. It appears that in these experiments, the substances evaporated carry away from the vessels with which they are in contact a part of their natural electricity; but when water was poured upon red-hot iron pans, the electricity was no more negative, as in the former experiments, but decidedly positive. These experiments were communicated to the Academy of Sciences in the year 1781.

M. de Saussure has also tried many experiments of this kind by plunging hot iron and other metals in water, and pouring water into crucibles of iron, brass, copper, silver, or porcelain. Sometimes he used distilled water, also spirits of wine and ether, and in these experiments the electricity was sometimes positive, sometimes negative, and sometimes neither.

M. de Saussure thinks that when the operation which converts the water into vapour at the same time decomposes it, or the body with which it is in contact, it produces a new quantity of the electrical matter, and that the vessel used in the operation becomes positive, negative, or neither, according as the fluid produced is superior, inferior, or equal to that which is taken from the vessel by evaporation. An account of these curious experiments is contained in the second volume of M. de Saussure's *Travels over the Alps*, page 227.

The gold leaf electrometer being well adapted to the performance of experiments of this kind, I was induced to repeat some of them with variations, in hopes of new appearances: and since almost every substance in the whole chemical nomenclature may thus be subjected to the action of fire, and its affinity with electricity in a state of vapour examined, new facts may yet arise which will produce new theories; therefore, the following experiments are placed in the order they were tried, without regard to system.

Experiment 1.—A bason of tinned iron about six inches wide at the bottom, and eight inches at the top, was placed upon the cap of a gold leaf electrometer. The bottom of the bason was covered with water about an inch deep. An iron chissel was heated red-hot, and dropped into the water, one end being immersed and the other resting on the edge of the vessel. The gold leaf gradually opened about an inch negatively, then closed and opened positively, remaining positive to the end of the experiment.

2.—The chissel was heated more than in the last experiment, and the gold leaf struck the sides of the electrometer six times negatively ; then it changed, and stood at half an inch positive to the end.

3.—The chissel was again made very hot, and it caused the gold leaf to strike often negatively ; after which it closed, but never opened positively.

4.—The chissel was heated very much by blowing the fire, and it caused the gold leaf to strike fourteen times negatively, but no positive electricity appeared. This was repeated about twenty times without any production of positive electricity.

5.—Upon observing that the chissel was much calcined upon its surface, by being so frequently heated in the above experiment, I rubbed off the calx and heated it again, and found that now it produced first negative and then positive electricity, as before ; whence it appeared that its production of positive electricity depended upon the metallic state of the iron.

6.—A large bar of copper and a piece of brass were heated red-hot and plunged in the water, which also produced first negative and then positive electricity.

7.—Melted lead was dropped into water contained in the bason as above, which gradually opened the gold leaf positively.

8.—Melted lead being dropped into a deep narrow vessel almost full of water, did not electrify it, since it emitted no vapour. It is, therefore, not the mere decomposition of the metal, but the formation of a certain kind of vapour, which excites electricity.

9.—Boiling mercury was dropped from an earthen crucible into a small quantity of water in a porcelain cup standing upon the cap of the electrometer, which caused the gold leaf to open positively.

10.—Several thin pieces of bell metal were heated red-hot and dropped into the water, which caused a negative repulsion only.

11.—An earthen unglazed flower pot was half filled with red-hot cinders ; then the other half was filled with chopped grass, which caused the gold leaf to open with positive electricity, and to continue striking the sides a considerable time. In the same manner were tried cabbage leaves, lettuces, turnip tops and roots, and chick weed, with the same results.

12.—Turnings of dry ash wood were thrown upon the hot cinders as above, which smoked much without causing any repulsion of the gold leaf till water was added, which caused a strong positive electricity.

13.—Dry hay was burnt in the flower pot, as above, which produced no electricity till water was added, and then it became strongly positive.

14.—The flower pot was placed upon the electrometer with red-hot cinders alone, and water was dropped into the middle, which opened the gold leaf negatively ; then, dropped near the side, opened it positively ; again in the middle, negatively.

15.—Since the cinders became more positive and negative with

water only, I repeated the above experiments on green and dry vegetables with a red-hot earthen crucible, but without any difference in the result. A square piece of brick was first placed upon the electrometer, to prevent the heat from injuring it in this and all the subsequent experiments, in which red-hot bodies were to be placed upon it.

16.—A spoonful of salt was thrown into the red-hot crucible, which opened the gold leaf negatively; a second quantity was thrown in, and it became positive.

17.—The crucible being again heated, two spoonfuls of salt were thrown in, which opened the gold leaf positively; then water was dropped into the middle of the salt, which changed its electricity to negative; then more water dropped near the side of the crucible opened the gold leaf positively; then again in the middle negatively.

18.—Salt and water were mixed and dropped into the red-hot crucible, which caused a strong negative repulsion; then, after the water was evaporated, more was dropped in the middle, which renewed the negative repulsion till the water was dropped near the side, when it became positive. This change from positive to negative was often repeated.

19.—A small quantity of salt was thrown into the red-hot crucible, which caused the gold leaf to open negatively; then the salt remained till it was burnt black, and water being added, caused a strong positive repulsion.

20.—The vitriolic and nitrous acids were severally dropped upon red-hot bricks, which opened the gold leaf positively. The bricks were tried before and after the experiments with water, which caused a negative repulsion.

21.—A large red-hot cinder was placed upon the electrometer, and olive oil dropped upon it, producing a copious smoke, but no electricity; then water was added, which opened the gold leaf negatively.

22.—Olive oil and water were shook together in a phial, and both poured upon a red-hot piece of cast iron about an inch thick, four inches broad, and five inches long, which opened the gold leaf strongly negative.

23.—Vitriolic and nitrous acid were dropped upon the cast iron, which opened the gold leaf positively.

24.—Dry hay was placed upon the cast iron, and pressed down by a stone placed upon it till it was burnt black; then water was added, which opened the gold leave positively. The hay was then brushed off, and more water dropped upon the iron, which caused the gold leaf to open positively till the surface was well cleaned and heated again, when it produced negative.

25.—A heap of wheat flour was placed upon the hot iron, which smoked without any electricity till water was added, which opened the gold leaf positively.

26.—Sand placed upon the hot iron, and water added, opened the gold leaf positively.

27.—Lump sugar placed upon the hot iron produced no electricity till water was added to the burnt residuum, which opened the gold leaf positively.

28.—Powdered charcoal was placed upon the hot iron, and water poured upon it opened the gold leaf positively; but when water was dropped upon a red-hot piece of charcoal, it caused a negative repulsion.

29.—Red and white port wine dropped upon the hot iron produced negative repulsion when the iron was so hot as to produce no electricity with water alone, which was tried before and after the wine.

30.—Raspberry wine dropped upon the hot iron produced positive electricity. The raspberry wine and red port were alternately dropped upon the same iron five or six times successively, which changed the electricity from positive to negative each time. Also several other sweet wines were tried, which, as well as sugar and water, produced positive electricity.

31.—Fresh urine poured upon the hot iron produced strong positive electricity.

32.—Milk poured upon the hot iron caused a strong negative repulsion; and even when water was added to the burnt residuum, except in some instances when it was burnt very black, it became weakly positive.

33.—Butter was burnt upon the hot iron, and water added caused a strong negative repulsion.

34.—Ale produced no electricity till water was added to the burnt residuum, which became positive.

35.—Dry tea leaves were burnt upon the hot iron, and water added opened the gold leaf positively; also black pepper and tobacco.

36.—Soap first burnt on the hot iron, and then water added; also soap and water mixed produced a strong negative repulsion.

37.—Tin-foil was placed upon the hot iron, which soon melted; then water dropped upon it first opened the gold leaf negatively, then weakly positive.

38.—Alum burning on the hot iron produced no electricity till water was added to the burnt residuum, which caused a strong positive repulsion.

39.—Moist salt of tartar was thrown upon the hot iron, which caused the gold leaf to open strongly negative; its vapours condensed so quick upon the tube used for trying the electricity, that it was with some difficulty that it could be excited.

40.—Saliva produced positive electricity. When water alone was producing negative repulsion, spitting upon it would immediately change it to positive.

41.—Yeast was strongly positive, but not till water was added to the burnt residuum.

42.—Cotton and linen rags were burnt on the hot iron, and water added, produced a weak positive repulsion.

43.—Sheep's wool, feathers, and hair, were burnt on the hot iron, and when a single drop of water was added, the gold leaf struck the sides of the electrometer positively.

44.—Rasped horn and bone were burnt upon the hot iron, which produced positive electricity when water was added, but not near so strong as in the last experiment.

45.—Powdered pit coal was placed upon the hot iron, which produced positive electricity upon the addition of water.

46.—Fresh sheep's blood was dropped upon the hot iron, which produced positive electricity.

47.—A hollow piece of bell metal about an inch thick, and three inches diameter, was cast on purpose to repeat these experiments upon, most of which were again tried, but with no remarkable difference, except that water produced negative electricity with more certainty. Upon the iron it was sometimes negative, sometimes positive, but mostly none at all; whereas on the bell metal it was almost always negative.

48.—Vinegar produced no electricity when dropped upon the hot bell metal, till water was added to the burnt residuum, which caused a strong positive repulsion.

49.—The bell metal was at last made too hot, and therefore broke to pieces as it was taken out of the fire; I therefore again had recourse to cast iron, and for the sake of its holding fluid substances, I turned a cast metal weight so as to have a very smooth concave surface in the middle, and a concave rim near the edge, so that I could place upon it several substances at once, without mixing. The lean part of a leg of veal was chopped small, and a small quantity placed upon the red-hot cast iron, which caused the gold leaf to open positively, but after burning some time, water was added, which changed its electricity.

50.—The fat of veal was burnt upon the hot iron, which produced no electricity till water was added, which caused a strong negative repulsion both before and after it was burnt black.

51.—Nitre was put upon the hot iron, which melted without producing electricity; then water was dropped upon it, which exploded without producing any electricity; sometimes small globules of water floated a considerable time upon the melted nitre, and then suddenly exploded without ever producing any electrical repulsion or gradual evaporation, as is usual with other substances.

52.—Bees wax produced strong negative electricity, with the addition of water.

53.—Verdigrease with water produced very weak positive electricity.

54.—Powdered antimony smoked much, and when water was added the gold leaf opened negatively.

55.—Caraway seeds dropped upon the hot iron caused a strong negative repulsion without the addition of water, also water added

when they were burnt black opened the electrometer negatively.

56.—Hemp seed thrown upon the hot iron caused a weak positive repulsion, but when water was added it became strongly positive.

57.—A tea spoonful of water was dropped into the concave hot iron which did not produce any repulsion, then some powdered gum arabic was added, which caused the gold leaf to diverge positively, then bees wax was also added, which caused it to become negative.

58.—These two last substances were dropped on the hot iron in the reversed order, which first produced negative and then positive.

59.—Mustard seed was dropped upon the hot iron, which caused no repulsion till water was added to the burnt residuum, which opened the gold leaf negatively.

60.—Linseed produced no electricity till water was added to the black residuum, which opened the electrometer weakly negative. The iron was then made hotter, and the addition of water produced positive.

61.—Canary seed also produced negative by the addition of water till it was burnt very black, and then it became positive; also anise seed, and fennel seed, were like the canary.

62.—Burnt millet seed produced positive electricity with the addition of water.

63.—Water added to burnt coriander and cummin seeds produced negative electricity till the residuum had continued burning a long time, and then it became positive.

64.—Hitherto the electricity of the apparatus whence the vapours ascend has been only examined. I now fastened a piece of paper upon a bent wire, which being placed in the cap of the electrometer hung over the hot iron, so that the vapours in ascending might pass over the paper, and shew whether this electrical state would in any instance be of the same kind observed in the vessel.

65.—The vapour of red port wine was tried as above, and found positive, contrary to the state of the iron in exp. 29.

66.—The vapour of raspberry wine was found to be negative contrary to the state of the iron in exp. 30. Also wine made from bilberries, and sugar and water mixed, produced negative vapours.

67.—The vapour of pure water was positive, but the addition of a small quantity of saliva changed it to negative.

68.—The vapour of raisin wine was first positive and then negative, and the reverse when the iron was examined.

69.—As carraway seed produced negative electricity without the addition of water, and therefore without any visible vapour, except a little smoke, the paper fixed upon the electrometer was several times placed at different distances above the hot iron, whilst the carraway seeds were burning, but no electricity appeared.

70.—The square brick which supported the hot iron was placed in the middle of an earthen plate full of water, and standing upon the electrometer, then the hot iron being laid upon the brick and out of the reach of the water, wool was burnt upon it, and a spoonful of water added; lastly, the whole was covered with a glass jar

with its mouth immersed in the water, that the vapour might not escape without touching the water; in this experiment no electricity appeared.

From these experiments it appears that various mineral, vegetable, and animal substances when evaporated, cause a positive or negative divergency of the electrometer, and it was remarked, in the beginning of this section, that M. de Saussure attributed these effects to the generation of a new quantity of the electrical fluid; but since the state of the electricity of the ascending vapour is contrary to that of the insulated vessel whence it rises, it now seems more probable that the vapour thus produced acquires its positive or negative state because its affinity with the electrical fluid is greater or less than that of the vessel, and that when a particle of vapour is but just in contact with the surface whence it rises, its capacity to become positive or negative is much greater than when it is entirely surrounded with air; and thus if the vapour becomes positive by absorbing electricity from the vessel, the vessel will become negative, and *v. v.*, without the necessity of supposing a new quantity of the fluid to be generated in this process.

The above extensive experiments of Mr. Bennet, which were, no doubt, made with great care, are not attended with those uniform results as might have been expected, from the views we have hitherto taken on this subject; and it is probable that those aberrations which are so conspicuous in several cases, might be traced to some other cause than that of simple evaporation; but as we have not yet repeated these experiments, any further remarks on them, in this place, would certainly be premature.

EXPERIMENTS BY DR. CHARLES SCHAFHAENTL.

The experiments of this gentleman were made in the Royal Adelaide Gallery, and in consequence of his considering that the electric phenomena of steam boilers being dependent on the incrustations which they contain, he selected one of Marcet's common boilers, in preference to one of Mr. Perkins's kind, in which incrustation is prevented. The boiler selected was globular, of iron, and about five inches diameter. In the vertical axis of this experimental boiler was a mercurial guage, and at the distance of about 45° from it, downwards, was a thermometer; and on the opposite side of the globe, a jet with a stopcock. The lower part of the cavity of the boiler was occupied by the mercury belonging to the steam guage, and on the top of this mercury an inch and a quarter deep of distilled water was placed. A glass bell, nine inches diameter, and five inches deep, was suspended with its axis in the axis of the jet continued, having the muzzle in the direction of the jet, at about four inches distance. The greater portion of a bundle of copper wire was inserted in the bell, and the remote end of the wire was connected with a gold leaf electroscope, furnished with a condenser. When the water in the globe had boiled to a sufficient extent to raise the mercurial gage to thirty-one inches, the stopcock of the jet was

opened, and the issuing steam condensed in the bell, and in a few seconds the gold leaves diverged to a great extent, with *positive* electric action, even without removing the insulated plate of the condenser. But when the steam was directed to the wire only, outside of the bell, no electric action was observable, for the very obvious reason that the condensation of steam was too trifling to show it.

When the water last used was drawn off, and replaced by a saturated solution of common salt, and afterwards boiled as in the previous case, no trace of electric action could be discovered. After this fact was ascertained, the boiler was cleansed of its saline contents, and filled with distilled water, which was boiled till the steam again arrived at a pressure of thirty-one inches of mercury; but in this case the electroscope could not be affected. When, however, the water was reduced to about the same quantity as at first, the condensed steam gave electrical indications. Several other variations of the experiments were made with various results, though in all cases where electrical indications were afforded, they were of the *positive* action. Dr. Schafhaentl was, however, led to suppose that the success of the experiments depended entirely upon that state of the escaping steam which caused a peculiar fluttering noise, which happened only when the boiler was charged with about an inch and a half deep of water; for when the steam ceased to make that fluttering noise, although at a pressure of thirty-two inches of mercury, would even cause the previously diverged gold leaves to speedily collapse.

This peculiar noise, which appeared to be so essential to the production of electricity by steam, Dr. S. supposes to be owing to the *sudden* boiling of the water, and the conversion of a portion of it into a fine spray; under which circumstances the issuing steam deposited a large quantity of water in the bell glass, which often ran in streams from its lower edge. Insulation of the boiler did not appear to affect the results of the experiments, the electrical action being to the same amount whether the apparatus was insulated or not. "A proof," says Dr. S., "that the electricity of the steam developed in the glass bell could not be *contained** in the steam during its passage through the three-inch long metallic jet of the boiler, as all the electricity would have been deposited in this narrow metallic passage." Dr. S. is led to suppose that a *partial* condensation of the steam into mist, for instance, is not sufficient to produce electric action, as its conversion into water appeared to be requisite for electrical development. From this circumstance the Doctor supposes that he has discovered some clue to direct him to the cause why *certain* clouds only, are capable of producing thunder storms. He says that "a common cloud, consisting only of moisture, seems to be analogous to a pure jet of steam in the glass bell, both consisting of minute hollow water globules, or bubbles, leaving only a small deposit of moisture in the glass bell, or in the air, which finally collects into small drops of rain. But when the steam depo-

* Of course Dr. S. meant that it was not *sensible* in that tube.

sits rapidly a great quantity of liquid water, which in a thunder cloud produces those well known heavy showers, electricity is set free in great quantities, so that a jet of issuing steam from Marcet's boiler, in three seconds produced the same effect on the gold leaves of the electroscope as a feeble spark from an electric machine, with a glass plate of nine inches diameter, produced in damp weather."

The general tenor of these speculations in the analogy, appears to be perfectly correct; at least it is in accordance with our own views on the subject; but we are very far from entertaining the opinion that the steam, whilst passing through "the three-inch long metallic jet," was not electrical. For such an opinion, if correct, would at once extinguish the idea, "that a *partial* condensation of the steam into mist, is not sufficient to produce electric action." For if the law of electric developement holds good in *one stage* of condensation, it must also hold good in every other stage. And, although the *misty* steam produced no sensible electric action in the experiments in question, that was no reason for supposing that no such action was present in the steam within the jet; but on the contrary, it would lead us to infer that the steam within the jet pipe was *electro-negative*, and its conversion into *mist* within the bell, brought it to its natural electric state of neutrality: and that a further condensation lessened its electric capacity another stage, by which means it became electro-positive, as shown by the experiments. These are the theoretical views we have always entertained on this interesting subject, whether the steam be produced and recondensed artificially, or by the usual processes of nature; and hence it is that we cannot entertain the idea of any dense cloud being in a *negative* electric state, unless influenced by the repulsive electric action of some other vicinal cloud more densely electrical than itself.

An experimenter would easily discover that Dr. S.'s electroscope was not of the most delicate kind, or its indications would not have been compared to those produced by a "spark;" for it is well known that a spark, however feeble, would immediately spoil a delicate gold leaf electroscope.

Although by some of the experiments already described, Dr. S. could not detect electric action when the boiler was charged with *salt water*, it is stated by that gentleman, that, in subsequent experiments, "if the water in the boiler was saturated with common salt, or with sulphate of lime, and even a slight excess of sulphuric acid, the angle between the diverging gold leaves remains the same as if distilled water was used." Hence, he very justly infers, "that the observed free positive electricity, in *this case*, was solely attributable to the sudden condensation and separation of water from steam, &c." And he might as justly have stated, that all *electro-positive* actions from steam or the vapour of water, depend on condensation. Hence, generally, whether the vapours be produced slowly or rapidly, it is *first* electro-negative, next, by a certain degree of condensation, electro-neutral, and, finally, electro-positive by every future degree of condensation.—EDIT.

ON VOLTAIC PILES.

Having so many solicitations from various quarters for information on several points in the history of electricity, and also respecting certain pieces of apparatus, especially in voltaic electricity, it would be difficult to know where to begin to give satisfaction to all parties. However, as some of our correspondents are anxious to know how to construct the *dry electric pile*, and others wish to have information respecting the earliest experiments in galvanism, we shall endeavour to satisfy both inquiries on these connected subjects, by giving the best data that we can procure from the accounts given by the authors themselves of their respective discoveries.

It appears that in the year 1767, a work, entitled "The General Theory of Pleasures," was published by M. Sultzer, in which the author describes the following experiment:—"Take two pieces of metal, one of silver, the other of lead, and bring their edges into contact with each other; and while thus connected, place them both on the tongue: a taste will be experienced similar to that produced by vitriol of iron (sulphate of iron). But no trace of any such taste is perceived by the application of either metal individually." Sultzer does not say whether he placed on the tongue each metal separately whilst in contact with the other, or whilst entirely alone; but it may possibly be useful to some readers to know, that the experimenter will find no difference in the effect, whether the metal placed on the tongue be in contact with another metal or not, provided this latter metal be not also in contact with the tongue, or some other moist part of the body. Under the latter circumstance, however, even by holding the second metal with the finger and thumb, if perspiring, or otherwise moistened, the peculiar taste will be felt. Although the experiment of Sultzer is of that class which has received the title "galvanic," it was not till about twenty-four years afterwards, viz., 1791, that Professor Galvani made his discovery, which led to this branch of electricity. The following account of this discovery, given by M. Volta in two letters to Cavallo, will always be found interesting.

The subject of these letters, says M. Volta, is that of animal electricity, discovered by Dr. Galvani, and published by him in a work entitled, "Aloysii Galvani de Viribus Electricitatis in Motu Musculari Commentarius. Bononiæ, 1791." This subject, under the name of galvanism, has given occasion to several important discoveries, having been very much cultivated by many respectable philosophers, and by none more than by those of England.

Dr. Galvani having cut and prepared a frog, so that the legs hung on one side of the spine of the back, separate from the rest of the body, solely by the crural nerves laid bare, he found that there were produced very quick motions in the legs, with spasmodic contractions in all the muscles, whenever a spark was taken from the prime-conductor of an electric machine, not on the body of the

animal, but on every other body, and in every other direction; this part of the animal being placed at a considerable distance from the conductor, and in certain circumstances. These were, that the animal, thus dissected, should be in contact, or very nearly so, with some metal, or other good conducting substance of a sufficient extent, and still better between two such conductors, the one of them being directed towards the extremity of the said legs of the animal, or some one of the muscles, the other towards the spine, or the nerves. It is also of great advantage that one of these, called the nervous and the muscular conductors, but preferably the latter, should have a free communication with the floor. It is in this position especially that the legs of the animal receive violent shocks, leap up and down rapidly on each spark from the conductor of the machine, though it may be pretty far distant, and though the discharge be not made on either the nervous or muscular conductor, but on some other body, likewise distant from them, and having another communication for transmitting such a charge, as on some person placed in an opposite corner of the room.

Such was the first step, which led him to the fine and grand discovery of an animal electricity, properly so called, appertaining not only to frogs and other cold-blooded animals, but also to all warm-blooded animals, as quadrupeds, birds, &c.; a discovery which makes the subject of the third part of the work quite new and interesting.

It was chance that presented to M. Galvani the phenomenon just described, but at which he was more astonished than he needed to have been, had he given due attention to the effects of electric atmospheres. Yet who could have believed that an electric current, so weak as not to be rendered sensible by the most delicate electrometers, was capable of affecting so powerfully the organs of an animal, and of exciting in its members, cut off many hours before, motions as strong as those of the living animal, as the vigorous springing of the legs, the leaping, &c., not to mention the most violent tonic convulsions?

M. Volta endeavoured to determine the least electric force requisite to produce these effects, as well in a living isolated frog as in one dissected and prepared as before-mentioned, which M. Galvani had omitted to do. M. Volta chose the frog in preference to every other animal, because it is endued with a very durable vitality, and is also very easy to prepare. He also made trial of other small animals for the same purpose, and with nearly the same success. Hence he found, that for the living and entire frog the electricity of a simple middle-sized conductor was sufficient, when it was only capable of giving a very feeble spark, and to raise Henly's electrometer to 5° or 6°. When he used a Leyden phial of a middle size, a much weaker charge produced the same effect, viz., such as gave not the least spark, and was quite insensible to the quadrant electrometer, and hardly sensible to Cavallo's electrometer.

All this was for a whole and isolated frog; but for one dissected

and prepared in different ways, especially after Galvani's manner, where the legs are attached to the dorsal spine only by the crural nerves, a still much weaker electricity, whether of the conductor or of the Leyden phial, the fluid being obliged to pass through the narrow passage of the nerves, never failed to excite convulsions, &c. Hence, then, we have, in the legs of the frog attached to the dorsal spine only by the bare nerves, a new kind of electrometer; since the electric charge which, giving no signs by other the most delicate electrometers, gives evident tokens of it by this new means, by what may be called the animal electrometer.

But if, after these experiments, we ought not to be surprised at those of Galvani described in the first and second parts of his work, how can we avoid being so at the very novel and marvellous ones in the third part? by which he obtains the same convulsions and violent motions of the members, without having recourse to any artificial electricity, by the sole application of some conducting arc, of which one extremity touches the muscles, and the other the nerves or the spine of the frog, prepared in the manner aforesaid. This conducting arc may be either wholly metallic or partly metallic, and partly some of the imperfect conductors, as water, or one or more persons, &c. Even wood, walls, the floor, may enter into the circuit, if they be not too dry. The bad conductors, however, do not answer so well, and only for the first moments after the preparation of the frog, as long as the vital forces are in full vigour; after which the good conductors only can be used with success, and soon after we can only succeed with the most excellent ones, namely, with conducting arcs wholly metallic. Galvani successfully extended these experiments, not only to many other cold-blooded animals, but also to quadrupeds and birds, in which he obtained the same results, by means of the same preparations; which consist in disengaging from its coverings one of the principal nerves, where it is inserted into a member susceptible of motion, in arming this nerve with some metallic plate or leaf, and in establishing a communication, by help of a conducting arc, between this arming and the depending muscles. Thus he happily evinced the existence of a true animal electricity in almost all animals. It appears proved, indeed, by these experiments, that the electric fluid has a continual tendency to pass from one part to another of a living organised body, and even of its lopped members, while they retain any remains of vitality; that it has a tendency to pass from the nerves to the muscles, or *vice versa*, and that muscular motion is due to a like transfusion, more or less rapid. Indeed it seems that there is nothing to be objected either to the thing itself, or to the manner in which M. Galvani explains it, by a kind of discharge similar to that of the Leyden phial.

M. Galvani following up the idea he had formed, after his experiments, and to follow in every point the analogy of the Leyden phial and the conducting arc, pretends that there is naturally an excess of the electric fluid in the nerve, or in the interior of the muscle, and a cor-

respondent defect in the exterior, or *vice versa*; and he supposes, consequently, that one end of that arc ought to communicate with a nerve which he considers as the conducting thread, or knob of the phial, and the other end with the exterior of the muscle. But had he but a little more varied the experiments, as I have done (says M. Volta), he would have seen that this double contact of the nerve and muscle, this imaginary circuit, is not always necessary. He would have found, as I have done, that we can excite the same convulsions and motions in the legs, and the other members of animals, by metallic touchings, either of two parts of a nerve only, or of two muscles, and even of different points of one simple muscle alone.

It is true that we succeed not quite so well in this way as the other, and that in this case we must have recourse to an artifice, which consists in employing two different metals, which is not necessary in experimenting after Galvani's method, at least, while the vitality in the animal, or in its amputated members, is in full vigour; but, in short, since, with the armings of different metals, applied either to the nerves only or to the muscles alone, we succeed in exciting contractions in these, and the motions of the members, we ought to conclude that if there are cases (which appears, however, very doubtful) in which the pretended discharge between the nerve and muscle is the cause of muscular motion, there are also circumstances, and more frequently, in which we obtain the same motions by a quite different way, a quite different circulation, of the electric fluid. Yes, it is a quite different sort of method of the electric fluid, of which we ought rather to say we disturb the equilibrium, than restore it, in that which flows from one part to another of a nerve, or muscle, &c., as well interiorly by their conducting fibres, as exteriorly by means of applied metallic conductors, not in consequence of a respective excess or defect, but by an action proper to these metals, when they are of different kinds. It is thus (says Mr. Volta) that I have discovered a new law, which is not so much a law of animal electricity, as a law of common electricity; to which ought to be attributed most of the phenomena, which would appear, from both Galvani's experiments and mine, to belong to a true spontaneous animal electricity, and which are not so, but are really the effects of a very weak artificial electricity. As to the motion of the muscles, my experiments, varied in all possible ways, show that the motion of the electric fluid excited in the organs does not act immediately on the muscles; that it only excites the nerves, and that these, put in action, excite in their turn the muscles. Whereas Galvani supposes in all cases, that the transfusion of the electric fluid, produced either by artificial electricity or by natural animal electricity, ought to act from the nerves to the muscles, or *vice versa*. But these ideas are restrained within too narrow limits; for, in varying the experiments in different ways, I have found that neither of these conditions, viz., the laying bare and isolating the nerves, but at the same time touching these and the muscles, to procure the pretended discharge, is absolutely necessary. It is sufficient, for

example, when we have laid bare the sciatic nerve of a dog, or a lamb, &c., to cause an electric current to pass from one part of this nerve to another, even next to it, leaving all the rest untouched and free, as well as the whole leg—it is sufficient, I say, for this, to see excited in this leg the strongest convulsions and motions; and this, whether we employ an artificial electricity, or put in motion the electric fluid in the nerve itself.

M. Volta next relates several experiments of his own, to prove these positions; and then adds, these last preparations lead to those of Galvani, which, indeed, prove that it is better to lay bare the nerves, and still more to detach them round about; but not that this is a necessary condition, since he had obtained the same convulsions and motions of the members by simply laying bare the muscles, and leaving all the nerves enveloped and hid under them in the natural state.

After these essays on reptiles, birds, and small quadrupeds, I proceeded (says M. Volta) to other and larger animals, rabbits, dogs, lambs, beeves; and not only produced the same effects by all the foregoing methods, but obtained some more remarkable and durable, as the vital heat subsists longer in these large animals and their members. For it must be remarked, that though in most cold-blooded animals, and particularly in frogs, vitality subsists in the amputated members many hours, this vitality, which renders them so sensible to the weakest electric irritation, lasts only some minutes in the several members of warm-blooded animals, and commonly disappears before all that animal heat is dissipated.

M. Volta having had such success in his experiments on large and small animals of all kinds, sometimes living and entire, sometimes skinned, decapitated, and dissected in different ways, also in each of their large members cut off, and almost always without Galvani's preparation of laying bare the nerves, he then wished to go further, and practise on the small members, on a muscle only, and on small pieces of muscles; which led to other new discoveries. Thus he cut off sometimes the leg with the thigh of a frog, sometimes the leg only, sometimes the half or quarter of a leg; and having applied, as usual, to a part of the lopped piece the tinsel, and to another part the silver plate, and made the armour communication, he always obtained motions and convulsions. Also the same with the legs or muscles, or any part of them, of a hen or other birds, rabbits, &c. Hence he infers that it is not at all necessary to make a discharge of electric fluid between a nerve and muscle, or to transpose it from the interior to the exterior of this latter by the nerve and the conducting arc, as Galvani supposes: and that it is useless to sustain here an analogy with the Leyden phial.

In like manner, in another experiment, having covered the two thighs of a frog, to exactly the corresponding parts, with two metal leaves, the one of silver, the other of tin, he excited the contractions of the muscles, and the usual motions of the legs as soon as he made a communication between those two armings by the conduct-

ing arc. But if two muscles, or two places in one muscle only, be armed alike, viz., with two plates or leaves of the same metal, equal in all respects, and applied alike, then connecting them by the conducting arc, there ensues no convulsion, no motion. But though these effects are constant and general in all quadrupeds, birds, fishes, reptiles and amphibia, which he has tried, it is not less true that worms in general, and many insects, have not the same effect. He tried in vain earth-worms, leeches, slugs, and snails, oysters and many caterpillars; being unable to excite any motion in these by small or moderate sparks, or discharges of artificial electricity. With some difficulty, however, he succeeded with cray-fish, beetles, shrimps, butterflies, and flies.

M. Volta found by his experiments that it was only the muscles subject to the will that are affected by them, and not those of the viscera, that are not usually so subject, as those of the heart, &c. He also shows that the electric fluid acts only mediately on even the voluntary muscles; that it is not even the immediate or efficient cause of the motion of those muscles; and that it is the nerves only that are directly affected, which again act on the muscles, viz., those more immediately connected with them: an assertion which, he says, is rendered evident, and proved by many experiments he had made on the tongue, which led him also to other curious and interesting experiments.

Having excited tonic convulsions, and strong motions, in the muscles and in the members, in both large and small animals, without laying bare any nerve, by the simple application of armings of different metals to the muscles stripped of the integuments, M. Volta began to think of attempting the same thing in the human subject. He easily conceived that it would succeed very well in amputated limbs; but how was it to be managed in the entire and living subject? It would be necessary to strip of the integuments, to make deep incisions, to remove even a part of the flesh where the metal plates must be applied. Happily it occurred to him that we have in the tongue a muscle naked, at least destitute of such thick integuments as clothe the exterior parts of the body: a muscle which is easily and voluntarily moveable. On this idea M. Volta made the following experiment on his own tongue. Having covered the tip of the tongue and a part of the upper surface, to the extent of some lines, with tin leaf, (silver paper is best), he applied the convex part of a silver spoon more advanced on the flat of the tongue, and inclined the spoon till its handle came in contact with the tin-foil. Thus M. Volta expected to see the trembling of the tongue, and for that purpose had placed himself before a looking-glass. But the expected motions did not take place; however, he felt instead of it a very unexpected sensation: a pretty sharp taste at the end of the tongue.

M. Volta was at first much surprised at the event; but on a little reflection, he easily conceived that the nerves which terminate at the tip of the tongue, being those destined for the sensation of taste, and not for the motions of this flexible muscle, it was quite natural that

the irritation of the electric fluid, moved by the usual artifice, should excite a taste there, and nothing else; and that to excite in the tongue the motions it is susceptible of, we must apply one of the metallic armings on its root, where the nerves destined for its motions are inserted, as in this following experiment. From a lamb just killed, having cut out the tongue near the root, he applied tin-foil to the part cut, and the silver spoon to one of its surfaces; then forming a communication in the usual way, between these two metal armings, he had the pleasure to see the whole tongue briskly agitated, raise the tip, and turn and bend up and down, all the time of the communication.

M. Volta repeated this last experiment on a calf's tongue, armed in like manner with the tin-foil and a silver plate, and with the same success. He repeated the same also on the tongues of various small animals, as mice, hens, rabbits, &c., and nearly always with the same effects.

ON THE ELECTRICITY EXCITED BY THE MERE CONTACT OF CONDUCTING SUBSTANCES. BY M. VOLTA.

From the Annales de Chemie.

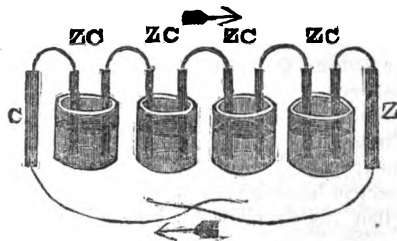
The chief of these results, and which comprehends nearly all the others, is the construction of an apparatus which resembles in its effects, viz. (such as giving shocks to the arms, &c.), the Leyden phial, and still better electric batteries weakly charged; acting continually; or whose charge, after each explosion, recharges itself again; which in short becomes perpetual, from one infallible charge, from one action or impulse on the electric fluid; but which besides differs essentially from the other, by this continual action which is proper to it; and because that instead of consisting, like the ordinary phials and electric batteries, in one or more isolated plates, or thin layers of those bodies deemed the only electrics, and armed with conductors or bodies called non-electrics, this new apparatus is formed only of a number of these last bodies, chosen even among the best conductors, and so the farthest removed, according to the usual opinion, from the electric principle. This astonishing apparatus is nothing but an assemblage of a number of good conductors of a different kind, arranged in a certain manner. Thus, 30, 40, 60, or more pieces of copper, or better of silver, each applied to a piece of tin, or still much better of zinc, and an equal number of layers of water, or of some other liquid which may be a better conductor than simple water, as salt water, lye, &c., or of bits of card, or leather, &c., soaked in such liquids. Of such layers, interposed between each couple or combination of two different metals, one such alternate series, and always in the same order, of these three kinds of conductors, is all that constitutes M. Volta's new instrument; which imitates so well the effects of the Leyden phial or electric batteries; not indeed with the force and explosions of these, when highly charged, but only equalling the effects of a battery

charged to a very weak degree; of a battery, however, having an immense capacity, but which besides infinitely surpasses the virtue and the power of these same batteries; as it has no need, like them, of being charged before hand, by means of a foreign electricity; and as it is capable of giving the usual commotion as often as ever it is properly touched. This apparatus, as it resembles more the natural electric organ of the torpedo, or of the electric eel, than the Leyden phial and the ordinary electric batteries, M. Volta calls the artificial electric organ. For the construction of this instrument, M. Volta provides some dozens of small round metal plates of copper, or tin, or best of silver, about an inch in diameter, like shillings or half-crowns, and an equal number of plates of tin, or much better of zinc, of the same shape and size: these pieces he places exactly one upon another, forming a column, pillar, or pile. He provides also as many small round pieces of card, or leather, or such like spongy matter, capable of imbibing and retaining much of the water, or other liquid, when soaked in it. These soaked rouletts or circles are to be a little less in diameter than the small metal discs or plates, that they may not jut out beyond them. All these discs are then placed horizontally on a table, one over another, continually alternating, in a pile as high as it will well support itself without tottering or falling down; beginning with a plate of either of the metals, as for instance the silver, then upon that one of zinc, over which is to be put the soaked card; then other three discs over these in the same order, viz., a silver, next a zinc, and then another moistened card, &c.

After having raised the pile to about twenty of these stages or triads of plates, it will be already capable, not only of affecting Cavallo's electrometer, assisted by the condenser, so as to raise it 10° or 15° , charging it by a simple touching, so as to cause it to give a spark, &c., as also to strike the fingers with which we touch the top or bottom of the column, with several small snaps, the fingers being wetted with water. But if to the twenty sets of triplets of the plates be added twenty or thirty more, disposed in the same order, the actions of the extended pile will be much stronger, and be felt through the arms up to the shoulders; and by continuing the touchings, the pains in the hands become insupportable.

M. Volta constructs and combines his apparatus in various ways and forms, more or less powerful, convenient, or amusing. One is as follows, Fig. 1, which he calls a *couronne des tasses*. He disposes in a row a number of cups, of wood, or earth, or glass, or any thing but metal, half filled with pure water, or salt water, or lye; these are all made to com-

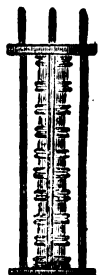
Fig. 1.



municate in a kind of chain, by several metallic arcs, of which one arm or link c, or only the extremity, immersed in one of the cups, is of copper, or of copper silvered, and the other z, immersed in the following cup, is of tin, or rather of zinc, the two being soldered together near the crown of the arch. It is evident that a series of these cups, thus connected together, either in a straight or curved line, by the two metals and the intermediate liquid, is similar to the pillar or pile before described, and consequently will exhibit similar effects. Thus, to produce the commotion or sensation in the hands or arms, we need only dip one hand into one of the cups, and a finger of the other hand into another cup, sufficiently distant from the former; then the action will be so much the stronger as the two cups are farther asunder, or have the more intermediate cups; and consequently the greatest by touching the first and the last in the chain.

As to the structure in the other method, by the column or pile, M. Volta found out various ways to prolong and extend it, in multiplying the metal plates without shaking it down, to render this instrument convenient, portable, and durable; and, among others, that exhibited on Fig. 2, upright bars or rods, to the number of three or four, or more, are erected from the bottom of the pile, and extended to a convenient height, inclosing the pile like a cage, to prevent its falling. These rods may be either of glass, wood, or metal; only, in this last case, they must be hindered from touching the metal plates; which may be done, either by covering each metal rod with a glass tube, or by interposing between them and the pile some bands of cerecloth, or oiled paper, or simple paper, or any imperfect conducting substance. But the best expedient for forming the instrument of a great number of plates, as of 60, 80, 100, is to divide the pile into two, or more.

Fig. 2.



M. Volta concludes with various remarks and cautions in using this instrument; showing that it is perpetual in its virtue, renewing its charge spontaneously, and serving most of the purposes of the ordinary electrical machines, and even affecting and manifesting its power by most of the human senses, viz. feeling, tasting, hearing, and seeing.

LETTER OF PROFESSOR VOLTA TO J. C. DELAMETHRIE, ON THE GALVANIC PHENOMENA.

SIR,

Paris, 18 Vendemiaire, Year 10.

You have requested me to give you an account of the experiments by which I demonstrate, in a convincing manner, what I have always maintained, namely, that the pretended agent, or *galvanic*

fluid, is nothing but common electrical *fluid*, and that this fluid is incited and moved by the simple *mutual contact of different conductors*, particularly the metallic; showing that two metals of different kinds, connected together, produce already a small quantity of true electricity, the force and kind of which I have determined; that the effects of my new apparatus (which might be termed electromotors), whether consisting of a pile, or in a row of glasses, which have so much excited the attention of philosophers, chemists, and physicians; that these so powerful and marvellous effects are absolutely no more than the sum total of the effects of a series of several similar metallic couples or pairs; and that the chemical phenomena themselves, which are obtained by them, of the decomposition of water and other liquids, the oxidation of metals, &c., are secondary effects; effects, I mean, of this electricity, of this continual current of electrical fluid, which, by the above-mentioned action of the connected metals, establishes itself as soon as we form a communication between the two extremities of the apparatus, by means of a conducting bow; and when once established, maintains itself, and continues as long as the circuit remains uninterrupted. You have requested this sketch from me, to be inserted in the next number of your *Journal de Physique*, convinced in your own mind of the truth of these observations by some of the experiments which I had the pleasure of shewing you yesterday with my small portable apparatus, in presence of the celebrated philosopher of Geneva, M. Pictet, and some other friends. I regret that I have not sufficient time to enlarge the essay which I send you, so as in some measure to comply with your invitation, and which can answer your expectations only in a partial manner. Accept it then as the precursor of a more ample memoir, which I intend shortly to publish.

I began by shewing you, by means of experiments, delicate indeed, but yet simple, that phenomena unequivocally electrical are produced by the mere contact of two different metals, without the intervention of any wet substance—experiments which ought to be considered as fundamental.

In order to render this electricity (which is so feeble that without other artifices it would remain imperceptible) sensible and manifest, I employ my electrometers with fine straws, combined with my condensers, the best of which are those made with two metallic discs, which apply very exactly to each other by their faces, which are very flat, and covered with a slight layer of sealing wax, or, which is better, of good lac varnish.

The first method of performing this experiment was to take two other discs or plates, the one of copper and the other of zinc; to hold each by an insulating handle (of glass covered with sealing wax); to apply them for an instant to each other by their flat faces, and afterwards separating them dexterously, to bring them into contact with the electrometer, which then indicated, by the divergence of its straws, to the distance of about a line from each other, the electricity which each of the plates had acquired; and whether

the electricity was positive (or el. +) in the zinc, and negative (or el. —) in the copper, as could be shown by approaching a stick of sealing wax, that had been rubbed, to the same electrometer.

It is proper to observe in this experiment, that the two plates, at the same time that they are *motors of electricity* by virtue of their mutual contact as different metals, perform also the function of condensers, because they are presented, the one to the other, by a large surface, in consequence of which their contrary electricities are counterbalanced in the best possible manner. This is the reason why this positive electricity in the plate of zinc, and negative in that of copper, which otherwise would not rise to more than about a sixteenth of a degree, and which in fact does not rise higher as long as these same plates remain applied the one to the other, elevates itself, on detaching them, to 1, $1\frac{1}{2}$, or 2 degrees, and even more.

This degree of electricity may appear trifling; it does not satisfy some persons, who always wish to see effects exemplified upon a large scale. Be it so. In order to obtain electrical phenomena much more distinctly marked, I generally employ a second condenser, mounted upon the electrometer itself, and proceed in the following manner:—I apply the plates of copper and of zinc to each other, and separate them several times, at each separation bringing one of these insulated plates into contact with the upper disc of the condenser, and the other, also insulated, with the lower disc, which is attached to the electrometer. When this contact has been repeated ten, twelve, or twenty times, the disc of the above-mentioned condenser being raised, the electrometer supports the inferior disc alone, which elevates itself to ten, twelve, fifteen, twenty degrees, &c., &c.

It might be imagined that, independent of the action of the condenser, the extent of contact between the two different metals greatly contributes, as such, to raise the electricity to the degree which we have seen, and that we should obtain a much inferior degree, if they touched each other only by a few points. But I prove the contrary; that is to say, that in the one case, as well as in the other, the electric tension rises, during the contact, to the same point, which is about a sixtieth part of a degree of my electrometers of thin straws, when the metals are zinc and copper, and a little more when they are zinc and silver; which tension requiring a quantity of electric fluid in the plate which performs the office of condenser, proportionably larger, accordingly as it condenses 60, 150, 200 times, it is evident why we obtain 1, $1\frac{1}{2}$, 2 degrees, &c., &c.

In order to prove that the contact of two metals of little extent, and which even subsists only at some points, displaces the electric fluid in such a manner as to raise the tension in these metals to the same degree; I join a small plate of copper with another of zinc, either similar or dissimilar with respect to figure and size, applying them to each other at a few points only, or at more points, or even soldering them together end to end.

Holding the piece of zinc with two fingers, or in any other

manner, I make the other, of copper, communicate with the superior disc of the condenser, whilst the interior disc communicates, as it ought, with the ground. A moment afterwards, raising this upper disc into the air, and holding it insulated, it gives me by the electrometer from two to three degrees of negative electricity (el. —), accordingly, as such a condenser condenses from 120 to 180 times. This proves that the electric tension of the above-mentioned plate was about a sixtieth of a degree, or nearly equal to that which the two plates of copper and of zinc acquired in the preceding experiments, when applied to each other by the whole extent of their flat surfaces.

When we reverse the experiment, that is to say, when we cause the plate of zinc to communicate with the condenser, we likewise obtain from two to three degrees, but of positive electricity (el. +).

However, if the disc of the condenser be copper, and the zinc touch it immediately without any intervening substance, we obtain nothing, because the zinc being then in contact, at the two opposite ends, with copper and copper, two equal forces act in opposite directions, and by that means destroy or counterbalance each other.

It is, therefore, necessary that the communication of the plate of zinc with the copper disc of the condenser should be effected by the interposition of a conductor, which should be a simple conductor merely, a humid conductor, such as a piece of wet card or cloth.

As to the rest, the action which excites and gives motion to the electric fluid does not exert itself, as has been erroneously thought, at the contact of the wet substance with the metal, where it exerts so very small an action, that it may be disregarded in comparison with that which takes place, as all my experiments prove, at the place of contact of different metals with each other. Consequently the true element of my electromotive apparatus, of the pile, of cups, and others that may be constructed according to the same principles, is the simple metallic couple, or pair, composed of two different metals, and not a moist substance applied to a metallic one, or inclosed between two different metals, as most philosophers have pretended. The humid strata employed in these complicated apparatus are applied, therefore, for no other purpose than to effect a mutual communication between all the metallic pairs, each to each, ranged in such a manner as to impel the electric fluid in one direction, or in order to make them communicate, so that there may be no action in a direction contrary to the others.

After having well ascertained the degree of electricity which I obtain from one single pair of these metallic pieces, as shown by the condenser which I employ, I proceed to show that with two, three, four pairs, &c., properly arranged, that is to say, disposed all in the same direction, and communicating the one with the other, by as many humid strata (which are necessary, in order that there may be no contrary actions, as I have already shown), we have exactly the double, triple, quadruple, &c., so that if with a single pair we

should be able to electrify the condenser to that point as to make it indicate, by the electrometer, three degrees, for example, we should obtain six with two, nine with three, twelve with four, &c., if not exactly, yet nearly so. You have seen these experiments, and have appeared to be very well satisfied with them, as well as M. Pictet, who seemed enchanted with them, and was never tired with seeing them repeated.

Here then we have already constructed a small pile, which however does not yet afford any indications by the electrometer without the aid of the condenser. In order that it may immediately afford these, in order that it may arrive at one whole degree of electric tension, which it will scarcely be possible to distinguish, because it amounts to no more than half a line on the electrometer, it is necessary that such a pile should be composed of about sixty of these combinations of copper and zinc, or, which is better, of silver and zinc, on account of the sixtieth of a degree which each combination yields, as I have already remarked. It then also affords shocks if we touch its two extremities with wet fingers, and much stronger shocks if we touch them with metals grasped by large surfaces in the hands, well moistened, by which means a much better communication is established.

In this manner we may receive shocks from an apparatus, whether in pile or with cups, of thirty and even of twenty pairs, provided the metals be sufficiently clean, and especially if the humid strata be moistened, not with mere pure water, but with a considerably strong saline solution.

These saline liquids, however, do not properly augment the electric force—not at all—they merely facilitate the passage, and leave a freer course for the electric fluid, being much better conductors than simple water, as several other experiments prove.

In order to establish this fact, and render it evident to such as have found a difficulty in admitting it, that the electric force or tension is, if not entirely, yet very nearly the same, whether the wet layers be moistened with pure water or with a saline solution, though the difference in the shock is so great, I have frequently made the following experiments, of which I have spoken to you, and which I would have shown to you, if I had been provided with the requisite articles. I take thirty cups or drinking glasses, with which I construct one of these apparatuses which I term a crown of cups, putting into them a sufficient quantity of pure water, and causing them to communicate, the first with the second, the second with the third, and so on successively to the last, by means of metallic wires, which terminate at one end in a plate of copper, and at the other in one of zinc, and are all turned in the same direction. The apparatus being constructed in this manner, I try its electric force, by causing the first of the cups to communicate with the ground, and applying the condenser to a piece of metal which is partly immersed in the last: this condenser, when I afterwards withdraw it, and separate one of its discs from the other in the proper manner, and without

delay, gives me forty, sixty degrees, and more, according to its condensing force. I also try the shock in the most advantageous manner, and find that it is very slight: after having well ascertained both the degree of the electricity and the weakness of the shock, I add a pinch of salt to each cup, and repeating the proofs, I find that the electricity has not been at all increased, the condenser giving me still only forty or sixty degrees, as before: but the shocks are incomparably stronger.

There are many other experiments which I have described to you verbally, and which I would willingly have performed before you, had I not been in want of the requisite apparatus. I informed you—at which you were much astonished, and M. Pictet still more so—that with one apparatus I charge a Leyden phial, whatever its capacity may be, and even a large battery; that I charge them in an instant, or to speak more accurately, in less than a twentieth part of a second, and almost to the same degree as the apparatus itself, namely, to about one degree of tension, if the apparatus be composed of sixty pairs; to two degrees if it contain one hundred and twenty, &c.;—that I am then able to draw, by the help of the condenser, a strong spark from a small jar charged in this manner, a great number of similar sparks from a large jar, and almost without limit from batteries, as I am actually able to draw them without limit of number from the apparatus itself.

I have informed you that large bottles charged in this manner gave me moderate shocks, and batteries pretty strong ones, as high as the elbow and higher; that the shocks of a battery of ten square feet of coating, and charged in less than a twentieth part of a second by one of my apparatuses, of two hundred metallic pairs, are very violent and almost insupportable; for I have not yet made any trial with larger batteries; but that there is every probability that the violence of the shocks increases with the size of the batteries, as far as a certain term, which I am not able to define; so that it would be possible, with batteries of 40, 60, and 100 square feet, to give considerably strong shocks, by charging them by the transient contact of a pile only 60, 40, 30, or still fewer pairs.

I have explained to you the manner in which one ought to proceed in order to perform these experiments with success; that it is particularly necessary we should carefully avoid the slightest interruptions in the communications of the conductors with the coatings of the bottles, and between each other, and that this becomes still more necessary when the electromotive apparatus, being composed of a small number of pairs, possesses but little power, so as to be unable to overcome the slightest obstacle that might oppose the passage and the course of the electric fluid.

Lastly, I remarked to you that these experiments confirm in a very evident manner what all the others already suggested, namely, that the quantity of electric fluid set in motion by my apparatus is much larger for every moment of time than that which is set in mo-

tion by the ordinary electrical machines; that the former afford it in much greater abundance than the latter, when the object is to produce, not an accumulation of electric fluid in insulated bodies, in order to raise the electricity to a high degree of tension, which may be done with those machines, but by no means with piles and other similar apparatuses, unless we also employ condensers; but where we require a constant current of this fluid, supported by a continued action of a circle of condensers not insulated; nay, one of my apparatuses, of only sixty or thirty metallic pairs, pours out in every instant, or in any given time, more electric fluid, if it meets with no obstacle, or if the fluid be not obstructed by the too small capacity of the recipient into which it is infused, than one of the most active electrical machines with cylinders or plates of glass. In fact, where shall we find a machine capable of charging a very large battery, to one, or even half a degree, in less than an eighth of a second, of pouring into it a sufficient quantity of electric fluid to enable us afterwards to draw from it, by the condenser, a great number of sparks in succession, as is done by one of the above-mentioned apparatuses?*

The other experiments, which I was able to show you in part, relate to the different electroscopic phenomena which the apparatus presents, accordingly as the one or the other of its extremities communicates with the ground, or both of them, or neither one nor the other, or as they communicate only between themselves and with the ground at the same time; accordingly as these communications are effected by perfect conductors, conductors more or less imperfect, &c., all which circumstances singularly modify and produce great variations in the results, which often appear curious and even anomalous, but which, nevertheless, I think myself able to explain in a satisfactory manner, without deviating from my principles and sound electrical theory, attention being paid to the mode in which the imperfect or bad conductors act. It would carry me too far to enter at present into these details; what you have already seen, and what I have communicated, may be sufficient for the present occasion.

[The electricity developed by the simple contact of bodies, was first discovered by the Rev. Abraham Bennet, prior to the year 1789. This ingenious philosopher, who is also the inventor of the electrical doubler, has published an extensive series of experiments on the contact of metals; which, although not introduced in the proper chronological order in this historical sketch, shall be made known to our readers before we quit the subject.—EDIT.]

* On this subject see *Philosophical Journal*, 4to, IV, 243—245.

EXPERIMENTS ON THE ACTIVITY OF A GALVANIC PILE. BY M.
DYCKHOFF.*

In some observations of Mr. Ritter on galvanic actions,† he has stated that all those substances which are placed between the metallic elements of the pile, are active in exciting electricity only in consequence of their containing moisture; and asserts that it is impossible to construct an active galvanic pile independently of wet substances. I take the liberty of placing in opposition to this doctrine, the following novel facts:—

I constructed a pile with discs of copper and zinc, and small pieces of thin green glass about the size of a lentil, three of which I placed at equal distances from one another, in each interval between the metallic pairs; by which means I had as many thin strata of air, instead of moist substances, in every metallic interval in the pile. The following results were obtained by this apparatus:—

1. A pile of ten pairs, assisted by the condenser, affected the electrometer as powerfully as it was affected by a wet pile of five pairs, of the ordinary construction.

2. The action continued without variation, during the time the air remained quite dry; but damp air appeared unfavourable to the electricity of the pile.

3. The same degree of electric action as that shown by the pile alone, was communicated to a Leyden jar.

The plates were only three inches diameter; and as they had previously been employed in a pile of the ordinary kind, and were much corroded by the liquid, I took off the oxide with a file. The plates, however, still remained rough and uneven on their surfaces, which was probably the reason of my not obtaining a higher degree of electric action. I have not hitherto had an opportunity of carrying on the experiments on a large scale, but the results already obtained are, of themselves, of sufficient importance to place the views of Ritter in a questionable position, at least. For my own part, I can see no reason why glass, thin films of sealing-wax, and other similar substances, should not have the same influence in the *pile* of Volta as in his *condenser*. In order to ensure success in the experiments I have already described, it is necessary that the bits of glass be as thin and smooth as is possible to procure them; for if the film of air be too thick, it is natural to suppose, that the feeble electricity excited by the simple contact of the metals, would be too insignificant to affect the adjacent metallic pairs in a degree sufficiently active to become conspicuous.

[The next form of the dry electric pile was made by M. Marechaux, in the year 1806, as will be understood by the author's own description of it in the following paper.—EDIT.]

* Voigt's *Magazin fuer den neuesten Zustand der Naturkunds*, vol. iv, p. 791.

† *Intelligenzblatt der Allgemeinen Litteraturzeitung*, 1802, No. 193.

NOTICE OF SOME EXPERIMENTS MADE BY THE GALVANIC SOCIETY
AT PARIS.*

I. M. Marechaux, of Wesel, correspondent of the Galvanic Society, announced to them that he had determined that water, whether pure or mixed with an acid, or charged with any salt, is not indispensably required for producing the effects of galvanism. He added, that some time ago he had constructed columns of zinc and brass with the interposition of discs of card, *not moistened*, which were very useful. The Galvanic Society was of course desirous of verifying a fact of this nature, and determined to repeat the experiments of M. Marechaux, as described in his letter.

Discs of zinc, which had been before used, were cleaned and restored to their usual polish. Similar pieces were made out of new brass. A vertical column of forty-nine pieces of discs was formed by the interposition of pieces of card, *not moistened*, standing upon a plate of brass, of greater dimensions, having three holes near its edges, through which cords of silk were passed in order to support the whole apparatus. These cords were tied together at the top, and the whole column suspended by them. This pile, which M. Marechaux distinguishes by the name of *colonne pendule*, was put into communication with the electric micrometer of M. Marechaux, simplified by M. Veau de Launay,† and it manifested an intensity of 360 degrees,‡ which was ascertained to be the effect of galvanic action, and not from the electricity of the atmosphere.

This first experiment was repeated and varied in different ways. Blotting paper was substituted instead of the pieces of card to the number of four for each, and there was no effect produced. Discs of card, dried in the oven were used, and the mean term of attraction in several experiments was 372 degrees. With the same pieces and twenty-five pair of discs only, the attraction was 160. The experiment was afterwards made with a column having the same number of pairs of metallic discs, but without the interposition of any pieces of card. In these circumstances no effect was produced.

These first results would have been sufficient for the society to confirm the fact announced by M. Marechaux, and which was intended to be verified; but this galvanic action of the pendulous column was not proved but by the help of an instrument of very great sensibility, and with regard to quantities scarcely to be estimated. It remains, therefore, for the society to ascertain the

* Annales de Chimie. Jan. 1806.

† See Journ. de Phys. Messidor, an. XIV.—See also Nicholson's Journal, XIV, p. 350.

‡ By intensity we denote the measure of the distance, at which a leaf of gold, suspended to a vertical stem of brass, is attracted towards another stem of the same metal, terminating in a ball, when these two stems are in communication with the two poles of the pile. Each degree of this measure answers to the eighteen thousandth of an inch.

advantage which it is possible to derive for the progress of galvanism by means of a discovery so important, by employing more powerful modes of action, and by comparing them with the effects obtained from piles excited by humidity or by saline solutions. The class of the society which is employed on physical researches, has been charged to direct its investigations.

II. A notice appeared in the *Moniteur* of the 22nd of Brumaire last, that Dr. Joseph Baronio, of Milan, had published a description of a galvanic pile formed of vegetable matters only, with an invitation to philosophers to repeat and vary his experiments, flattering himself that they would serve to extend the application of the theory of galvanism to the whole of vegetable life. The Galvanic Society was called upon to answer this observation of Dr. Baronio.*

A pile was accordingly formed by them in his manner: sixty equal discs of walnut-tree were made, two inches in diameter, having a raised edge of one-eighth of an inch high. These pieces were boiled in vinegar, and with these, and round pieces of raw beet-root, and of a thick raddish, (*raphanus sativus* of Linnæus,) a pile was constructed of sixty couples of pieces of beet-root and raddish, separated by discs of wood, on the upper extremity of each of which was poured, by means of the border, a solution of the acidulous tartrate of potash in vinegar. Lastly, at the lower extremity of the pile was placed a leaf of cochlearia, and at the upper extremity a double band of blotting-paper, steeped in vinegar. Everything being thus disposed agreeably to the full description inserted in the *Moniteur*, frogs, properly prepared for the action of this pile, were placed with the leaf of cochlearia in contact with their spinal marrow, and the band of paper with their muscles. Three frogs being thus successively and repeatedly presented, showed not the least motion, though they were sufficiently sensible to be strongly agitated when being supported on a knife to bring them near the conductor of the pile, they were in contact with the blade or silver mounting of the handle. After having made every probable experiment with these frogs without success, the pile was brought into communication with the electro-micrometer, upon which also it produced no effect. The same instrument was then presented to a pendulous pile, constructed after the manner of M. Marechaux, composed of sixty pair of new discs of copper and of zinc, with the interposition of pieces of card not moistened. The intensity was about 180 degrees. At the same moment, the frogs which had been presented to the vegetable pile, were put into communication with this last, and they gave no indication of sensibility.

The Galvanic Society did not, therefore, obtain in the experiments indicated by Dr. Baronio, the results which he announced; but they have served to show that the electro-micrometer made use of is still more sensible than frogs to show the smallest effects of galvanism.

(To be continued.)

* Annales de Chimie. Jan. 1806.

Researches on the Laws of Induction of Currents produced by Currents. Second Memoir. By M. ARRIA.*

THE experiments of this second memoir have been made by following the method pointed out in that which I had the honour of addressing to the Academy on the 17th of May last, which method consists in submitting the same needle, successively, to the action of each of the two currents—the induced and the inducing one, by the aid of helices equally inserted in the circuits. When the needle is suitably chosen, the inverse relation of the durations of oscillation is equal to that of the two currents.

I shall now give the principal consequences of my investigation.

1st. The intensity of each of the induced currents, direct and inverse, is proportional, all things being equal, as to the number of elements of the inducing current, which acts on the induced system : it is independent of the section of these same elements ; or, in other terms, directly proportional to the quantity of electricity which traverses them.

It varies in the direct ratio of the conductibility of the induced wire, and is equally divided amongst the different elements of this latter, when they are disposed in a similar manner in relation to the inducing wire.

The verification of these laws may perhaps be regarded as a confirmation of the method employed to ascertain the degree of intensity of induced currents. I may add, that this process gives the same values, in the limits of my experiments, for the relation of the two induced currents, whether they be determined directly, or whether we value each of the two by taking the induction for unity.

2ndly. The effect of induction decreases in proportion as the distances of the two systems is augmented, following a law which increases the rapidity as this distance becomes more considerable ; in such a manner that when the two systems are brought very near to each other, the intensity of the induced current is almost as strong, independent of their mutual distance ; this latter augmenting, the intensity decreases in inverse ratio, at first, of the square root of the distance, and afterwards, of the simple distance. The law of the inverse ratio of the square root of the distance is observed too when the two systems are even so much removed from each other that the distances of their different elements vary in the same relation.

3rdly. If we employ, for the induced system, a spiral of a single turn, made with a wire, the section, or the diameter of which we successively augment, the intensity of the direct current augments from a diameter of half a millimetre to that of two and a half millimetres, which is the largest I have employed, at first, as the square root of the diameter, or the fourth power of the section ; afterwards it is less rapid. The inverse is, in the same circumstances, smaller than the direct one, but the relation which the first bears to the second increases as the square root of the diameter, in such a man-

* Comptes Rendus.

ner that the inverse varies in direct ratio, at first of the diameter, and afterwards of its square root.

The intensity of each of the induced currents augmenting less rapidly than the section, it follows, in the case of an induced conductor, formed by the union of several wires, that there operates amongst them, when they are submitted simultaneously to the action of an inducing current, a reaction, by virtue of which, the quantity of electricity devolved in each one is less than when the others are taken away. If we give to the induced spiral two or three turns, in place of a single one, the effect of induction will become diminished in proportion as the number of turns is augmented, because of their mutual reaction. Observation justifies this consequence. We see, further, from it, that conformably to the facts which have been indicated by the experiments made with spirals of a single turn, that the inverse current decreases less rapidly than the direct one, in such a manner that the relation of the first to the second is greater (the section of the wire remaining constant) for a spiral of several turns than for one of a single turn. I have found, for example, 0^{mm} , 40 for the relation of two currents in a spiral of several turns in a wire of 0^{mm} , 64 in diameter, and 0^{mm} , 33, with a spiral of the same wire, of a single turn. When employing a wire of two and a half millimetres, I obtained for the two values of the same relation, 0^{mm} , 68, and unity almost, following, that the induced spiral consisted of one or more turns.

4thly. The two currents, direct and inverse, developed in the same system induced by the action of a similar inducing system, the first at the moment when the current from the pile is broken, the second at the moment when it is closed, differ from each other, not only by the direction, by the intensity, by their unequal increasing when the section is augmented, but also by their *tension*; the first of these two currents becomes enfeebled, following a law less rapid than the second, in its passage, across the wires of variable length and section inserted in the induced circuit.*

5thly. If we submit two spirals to the action of an inducing current, the intensity of the current induced in each of them is less, the other spiral being closed, than when it is open. The effect of this is null in this latter case; if we cause the same conductor to be traversed simultaneously by the induced currents of the two spirals, we find that they possess the property, like those of a limited duration, of joining themselves together, or of separating, according as

* If we establish the communication between the two extremities of the induced wire, by the aid of gold leaf, we can perceive the sparks which accompany the formation of the induced current with a spiral of thick wire, for which the relation of the inverse current to the direct one is almost unity. I have observed that there is no production of light except at the time of breaking the inducing current, in consequence then of the formation of the direct current. The inverse current, though equal to the direct one, undergoes a less powerful diminution in its passage through the gold leaf, and it is, without doubt, for this cause that there are no sparks in the induced circuit at the instant when the inducing current is completed.

the current is transmitted in the same or in the opposite direction. The verification of this property further confirms the exactness of the experimental method employed in these researches, and furnishes the means of verifying, in a great number of cases, the values which direct observation furnishes, for the intensity of induced currents.

6thly. In order to account for the diminution of the induced current in a wire or in a spiral, when other wires or other spirals are submitted to the same inducing cause, it is necessary to seek to know in what manner these inducing currents act on closed conductors placed in their neighbourhood, and submitted solely to their action. M. Henry was the first who occupied himself with this question, I shall here give the results obtained by him.

If we call the primary current the inducing current, it determines at the instant that it is broken in a neighbouring wire a current of the second order, which has the same direction as that of the first order, or primary one. If we cause this secondary current to act on a third conductor, closed, and not subject to the influence of the first, it will create a current of the third order, or tertiary current, the direction of which is opposed to that of the secondary current. This current of the third order gives the same development when acting on a current of the fourth order, by giving it a direction contrary to its own, in such a manner that we have, for the succession of the signs of different induced currents, the following series :
 Current from the pile, or primary current..... +
 1st current induced by the rupture of the preceding one, or
 secondary current..... +
 2nd current, (or tertiary current)..... —
 3rd current, (or current of the fourth order)..... +
 etc.

I have obtained up to the seventh order, the same results as M. Henry, however small the distance was between the inducing and the induced systems. I note this circumstance, because the skilful philosopher of New Jersey thinks that the induced current is not in a contrary direction to that of the inductor (in the case where this latter is itself an induced current), except when the distance of the two systems have acquired a certain value. I have observed, further, that there are analogous phenomena produced at the moment that the voltaic circuit is completed.

The series corresponding to the preceding one is in this case—
 Current from the pile, (or of the first order)..... +
 1st current induced at the moment when the preceding one is
 established, (or of the secondary order) —
 2nd current, (or of the tertiary order)..... +
 3rd current, (or of the fourth order)..... —
 etc.*

* If M. Arria had consulted the 6th volume of these *Annals*, he would have found all his experimental results predicted in our letter to Dr. Henry.
 —EDIT.

The instantaneous currents are distinguishable then, from those of a limited duration, inasmuch as they induce currents whose direction is contrary to their own, whilst that, for the others, the effects from the moment that their action commences are contrary to those which are of a continuous character.

That granted, when an inducing spiral acts on two spirals, A and B, it develops in each of them two currents of the second order and of the same direction; each of these latter create in the other a current of the third order, the direction of which is opposed to that of the current of the second order. We may conceive why it is that the quantity of electricity developed in each of the two spirals is smaller than if the other did not exist; but we may ask whether the current induced in each of the two spirals, in A, for instance, (the other being closed) is the difference between the current of the second order which determines the inducing spiral, and that of the third order which created the current of the second order of the spiral B. Though I am not yet in possession of the laws, following which these reactions are exercised, the very carefully made experiments which I report in detail in my memoir, establish, I think, in an incontestible manner, that the diminution of intensity produced in the current of each spiral by that of the second does not depend solely on the intensity of this latter one, and that it is necessary to regard some other circumstances, such as the relative position of the spirals with respect to the inductor.

Observations and Experiments on Light. By SAMUEL ADAMS, M.D., Professor of Chemistry and Natural History in Illinois College, Jacksonville, Ill.*

From Silliman's *American Journal of Science*.

SOMETIME in July, 1838, while on a mineralogical excursion, I accidentally noticed the wing-feather of a bird lying upon the ground; and being struck with the delicacy of its tints, I took it up to examine it. Observing that the vane of the feather appeared very thin

* To the Editors of the *American Journal of Science and Arts*.

Messrs. Editors—When "Observations and Experiments on Light" were forwarded to you for publication in the *Journal of Science*, I was not aware that Fraunhofer had anticipated the leading investigations of that communication. Pressing engagements, and frequent attacks of intermittent fever, prevented me from making so full an examination of the works of others on the subject as was desirable. I have since ascertained, that Fraunhofer has anticipated the leading results of my observations, in a series of experiments made by him by passing a beam of light through gratings, and examining the spectra produced through a telescope. (Herschel on Light, § 740, et seq.) I do not find, however, that the effect of the feather upon light has been before noticed, or that Fraunhofer ever exhibited the spectra upon a screen. You will oblige me by appending this as a note to my communication. Yours, &c.

Illinois College, May 21, 1841.

SAMUEL ADAMS.

and nearly transparent, I held it between my eye and the sky, which was very clear with the exception of a few fleecy clouds that contrasted finely with its rich blue. I was very much interested to observe that the clouds and all light coloured objects, which were highly illuminated, were seen through the vane of the feather beautifully fringed with the colours of the rainbow. I supposed that this phenomena depended upon the peculiar structure of the vane of the feather, and intended to investigate it as soon as I could find leisure. I did not however resume the subject till accident again called my attention to it.

About the 20th of June, 1839, while walking in the college grove, I happened to observe lying upon the ground some wing feathers of the jay, which reminded me of my former experiment. I collected the feathers, and after observing the same phenomena that I had noticed on the former occasion, I held the vane of the feather between my eye and the sun, and was greatly surprised at the gorgeous display of coloured spectra that were seen through it, arranged in the most exact mathematical order. The sun was seen in its natural position, slightly tinged with red, with its brightness considerably dimmed, and formed the intersecting point of two rows of coloured spectra, that crossed each other nearly at right angles. One of the rows of spectra formed a very acute angle with the shaft of the feather at its outer extremity, and the other was nearly at right angles with the shaft. In each coloured spectrum the side nearest to the sun was a mixture of violet and the contiguous rays of the prismatic spectrum, while the side furthest from the sun was uniformly red. The sun was slightly clouded when I made my first observations. Afterwards, when the sun shone perfectly clear, I observed that the angular spaces formed by the intersection of the two rows of coloured spectra were occupied by less brilliant spectra, arranged in the same order as the two rows above described.

On Monday, the 1st of July, 1839, I varied the experiments above described, by making my observations upon the flame of a lamp, instead of the sun. I found an advantage in this, as it enabled me to change the distance of the luminous object at pleasure. In looking through the vane of the wing feather of the wild pigeon at the flame of the lamp, I observed spectra coloured and arranged similarly to those which I saw when looking at the sun. I first looked at the lamp at the distance of eight or ten feet, and saw the two rows of coloured spectra above described entirely distinct from each other, with some faint appearances of spectra in the angular spaces near the lamp. As I approached the lamp (holding the feather to my eye and looking at the flame), the coloured spectra in the two rows gradually approximated to the flame of the lamp and to each other, their colours at the same time becoming less distinct and approaching to white light, while the spectra in the angular spaces became more perceptible. As I receded from the lamp, the spectra in the two rows receded from the central flame and from each other, their

colours at the same time becoming more distinct, and the spectra in the angular spaces gradually fading away.

My next step was, in connection with my colleague, Prof. Sturtevant, to introduce a small beam of light into a dark room by passing it through the vane of the wing feather of the jay. We observed coloured spectra arranged upon a screen in the manner described above. In the experiments which I first performed, the eye was the dark chamber and the retina the screen.

From reflecting upon these phenomena and conversing with Professor Sturtevant upon the subject, I was convinced that they were to be referred to diffraction, produced by the passage of light through the minute foramina formed by the crossing and interlocking of the barbules of the feather. This conviction was strengthened by a microscopic examination of the vane of the feather, which exhibited an extremely minute lattice work between the barbs of the feather, formed by the crossing of the barbules, and by noticing that the lines in which the coloured spectra were arranged were perpendicular to the bars of the lattice. The similarity between the arrangement of the colours in the spectra upon the screen, and those of the external fringes produced by diffraction, could not fail to be observed, and to incline me to the opinion that the law of interference, established by Dr. Young, had something to do with the production of the chromatic spectra. I was confirmed in this opinion by a series of experiments and measurements performed by Professor Sturtevant and myself, by which we ascertained that corresponding spectra received upon a screen at different distances from the feather, were not arranged in straight lines, but in curves. The curves seemed to belong to the hyperbola, and the latter to be formed by the section of a very acute cone. This is what might have been expected, as our experiments were performed upon parallel rays.

In order to understand the application of the law of interference* to the production of coloured spectra by the feather, it will be necessary to recur to the fundamental facts of diffraction. Let it be borne in mind, that when a beam of light falls upon the edge of an opaque body, the rays which pass by the edge are divided into two portions, one of which is bent into the shadow of the opaque body, and the other is bent outward from the body. This separation of a beam of light into two parts is called *diffraction*. For the sake of brevity and clearness I shall, in my subsequent remarks, speak of those rays which are bent into the shadow of the opaque body as *inflected rays*, and of those which are bent outward as *deflected rays*, and I shall use the terms *inflection* and *deflection* in strict accordance with these definitions. The plane of diffraction is a plane passing through an inflected and a deflected ray which have diverged from the same point, and is always parallel to and passes through the unmodified beam of light. When the diffracting edge is a straight line,

* See *Interference*, in Brewster's Optics, and Herschel on Light.

the plane of diffraction is always perpendicular to a plane passing through the diffracting edge and the corresponding outline of its shadow. In an irregular or curved diffracting edge the same law will hold with regard to any indefinitely small portions of it, which may be assumed as straight lines.

I am aware that the terms inflection and diffraction are used as synonymous by many who have written upon the subject of light. But without the definitions and limitations which I have just indicated, I should be compelled to resort to circumlocutions, which might render ambiguous the explanations which I am about to give of the phenomena of the feather. Again, I am not aware that the law which regulates the position of the plane of diffraction has been stated by any other writer, although it is fairly inferrible from the facts which they have brought forward, as well as from experiments performed by myself, and which I hope to notice more fully in a subsequent communication. It will be seen in the sequel, that the law which regulates the position of the plane of diffraction determines the angle which the two rows of coloured spectra make with each other.

Let us now turn our attention to the lattice-work formed by the crossing of the barbules of the feather, and inquire how the light passing through a single opening would be affected. The openings of the lattice are of course one of the four varieties of the parallelogram. The angles of these openings differ in the feathers of different birds, and in different feathers of the same bird. Let $a b c d$ represent one of these openings; and let us suppose a  beam of light passing through it perpendicular to the plane of the paper. It is evident that each of the sides of the opening will be a diffracting edge; and if we take any two opposite sides ab , dc , the inflected rays of one side will be bent in the same direction as the deflected rays of the other, and will be liable to interfere with each other, and produce coloured fringes upon a screen placed to receive the diffracted light, and these fringes would extend on each side of the opening in a line perpendicular to the two sides in question. The same will be true of the other two sides, ad , bc and thus we should have two rows of coloured fringes, whose lines of direction would be perpendicular respectively to the parallel sides of the opening, and consequently crossing each other at angles equal to those of the opening. But a part of the light would pass through the centre of the opening unbent, and would form upon the screen a white image at the intersecting point of the two rows of coloured fringes. Thus it will be seen, that a beam of light passing through a single opening of the kind above described, would be divided into nine parts, four being produced by the inflection of the four sides, four more by the deflection of the same, and one being the remains of the beams that pass on unmodified. Now let us suppose that a beam of light, instead of passing through a single opening, passes through an extremely minute lattice, containing an indefinite number

of such openings, as in the case of the feather. As all the bars of the lattice are parallel respectively to those which surround each individual opening, it is evident that the general effect upon the beam will be the same as that of a single opening, with this difference, that the range within which interference would take place, would be greatly enlarged, by enabling the inflected and deflected rays from different openings to interfere with each other; and thus the fringes, which are scarcely perceptible when formed by a single opening or a single edge, become brilliant spectra, when a beam of light is passed through a lattice of the kind described. All this is realised in the experiments with the feather. It is proper to remark, however, that the central white image is probably not formed entirely of unmodified light, but is partly produced by light slightly inflected by the opposite edges of the bars of the lattice, and corresponding with the internal fringes, first explained by Dr. Young upon the principle of interference. It is not improbable, that some of the deflected rays fall within the central white image and add to its brightness. The faint spectra in the angular spaces may be explained by supposing that they are formed by light which has undergone two inflections or two deflections, or one inflection and one deflection, by two contiguous bars of the lattice.

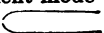
It should be noticed here, that all the coloured spectra, as well as the central white one, are considerably elongated in a direction perpendicular to the barbs of the feather. With a very delicate feather and a small luminous object, the eye can easily distinguish a row of coloured spectra arranged in the same direction. This is what might have been expected, and gives us some idea of the effect produced by passing a beam of light between extremely minute parallel bars arranged in the same plane very close to each other. The sun, moon, stars, the flame of a lamp, a small aperture in a dark room, &c., are convenient objects to be examined with the eye through the vane of a feather. When we wish to examine a luminous object through the vane of a feather, one of a dull or dark colour should be chosen, as a white feather transmits so much light as soon to exhaust the sensibility of the retina. For forming coloured spectra on a screen a white feather is preferable. Those feathers taken from the wing and tail, whose vanes approach the nearest to a plane, give the most regular arrangement of the spectra. The feathers of small birds, from the greater minuteness and delicacy of their structure, produce the most brilliant and extensive colours. We see here the same principle which Dr. Young applied to the construction of the Eriameter.* In looking through the vane of a feather at a bright object, the most brilliant spectra are seen on the side towards the outer edge of the feather. This may be owing to the thinning out of the feather towards the edge.

If the above explanation of the phenomena of the feather be correct,

* See Brewster's Optics.

it follows, that if an opaque screen be perforated with circular holes sufficiently minute and near to each other, it would produce a succession of coloured rings. When a beam of light passes through a lock of cotton, wool, or raw silk, inflection and deflection will take place in every possible direction, producing a blending of all the colours into white light in the centre, and a succession of coloured rings in receding from the centre. As in the feather there is a regular arrangement of the diffracting fibres, there is a corresponding arrangement of the coloured spectra. The explanation of the coloured rings produced by transmitting a beam of light through a lock of cotton, &c., applies to those produced by transmitting a beam through a plate of glass covered with fine particles.

Having satisfied myself with regard to the structure of the vane of the feather, and the mode in which it operates in producing coloured spectra, I concluded, that if that structure could be imitated by any artificial contrivance, the same effects might be produced as by the feather. I shall not detain the reader by detailing all the expedients which I resorted to in the subsequent course of my experiments, but will endeavour to indicate some modes of imitating the structure and effects of the vane of the feather more perfectly than can be done by any means which are at my command. Let it suffice to say, in the mean time, that silk cloth of a close and delicate texture, a dense gauze of fine wire, and similar contrivances, answer as clumsy substitutes for the vane of the feather.

The difficulty of obtaining the necessary materials, and of commanding the requisite mechanical skill, has prevented me from executing the most desirable plans, that have presented themselves to my mind. A convenient mode of arranging parallel fibres is, to bend a steel wire thus,  and wind a fine silk thread or delicate wire across its parallel sides. With a contrivance of this kind I was able to produce a row of spectra or fringes in a line perpendicular to the parallel fibres. I made use of a fine silk thread, but it is manifest, that fibres more minute, skilfully arranged, would greatly increase the brilliancy of the phenomena. In this case, it will be seen that the rays undergo two diffractions in the same plane. The second set of fibres would increase or diminish the effect of the first set, according as its diffracting influence coincides with, or counteracts that of the first; and it is probable, that both of these effects are produced upon different rays. A preferable construction would be to take a rectangular metallic frame, and wind the finest platinum wire across two of its parallel sides, so close as just to admit the passage of light between the parallel turns of the wire. The wire may be fastened by metallic bars screwed down upon it, where it crosses the exterior sides of the frame, and then one set of the parallel turns of the wire may be cut away, so as to leave only one set to act upon a transmitted beam of light. Two of these contrivances might be placed together and turned upon each other, so that the parallel wires in one could be made to cross those of the other at

any convenient angle, and thus the phenomena of the feather would be imitated. The crossing of the wires might be secured by winding the same frame in opposite directions, fastening the wires and cutting them away on one side in the manner above described. The first method, however, is preferable, as it admits the change at pleasure of the angles at which the two sets of parallel wires cross each other. This apparatus would probably be rendered more perfect by using fine hair, or wool, or better still, a single thread of the silk worm, instead of a platinum wire. As diffraction takes place at the edges of transparent as well as opaque bodies, probably an apparatus of the kind above described, made of very fine spun glass, would exceed all others in delicacy and power, as refraction in this would co-operate with diffraction. Since writing the above, Prof. Sturtevant has suggested the mode of fastening parallel fibres into a wooden frame by gluing pieces of wood upon its exterior sides. I have acted upon this suggestion and constructed an instrument with fine silk thread, which, though immensely inferior to the vane of the feather, produces phenomena similar in kind.

As my frontier location deprives me of the means of attaining the desirable perfection in the constructions which I have described, it is hoped that others more favourably situated, will be able to realise what I have hinted at above. It remains to be determined, whether art, in the construction of a diffracting instrument, will ever attain to that perfection which is presented to us by the hand of nature in the vane of the feather. Even with the latter we are able to render the chromatic effects of diffraction and interference as conspicuous to a class of students as those of refraction. Prof. Sturtevant lectured a few days ago upon the phenomena of the feather for the first time, to the great satisfaction of his audience.

During the progress of the above investigations, several inquiries have arisen, which Prof. Sturtevant and myself are now pursuing, and one or both of us may be expected to be heard from again upon this subject.

Illinois College, April 16, 1841.

*Researches on the Cause of the Movements which Camphor presents when placed on the Surface of Water, and on the Cause of the Circulation amongst Chara. By M. DUTROCHET.**

(First Part.)

1. THE progress which is daily being made in different branches of physical science, tends in general to unite them in a single group, and to show the unity which exists in the universe. Physiology alone holds herself apart, considering the vital forces as entirely

* Comptes Rendus, January 1st, 1841.

different to those forces which are obeyed by the inorganic world. For my own part, I have always thought that if the vital phenomena are not explicable at the present day by the means of physical phenomena, it is because the latter are not all known. Thus, for example, the forces under whose empire the liquids contained in the vegetable world are moved, ought, in my opinion, to be found in the physical world. Already has endosmosis revealed the existence and shown the mechanism of one of these moving forces, but endosmosis will not account for all the movements which the liquids contained in vegetables present. The movement of circulation which is observed in the central tube of each *merithalle* of *chara* and in the interior of the cellules of many plants, cannot at present be explained by any physical force known. This rotating movement in closed cavities, and the movement of the *latex* which takes place in the anastomosed vessels, and which has received from M. Schulk the name of *cyclose*, certainly both depend on the same moving force, but this force is altogether unknown, both in its nature and in the mode of its action. When studied amongst the *chara*, this force has been considered by certain physiologists as being of an electric nature; but there is nothing to justify this allegation, which is purely hypothetical. The researches which have been made in common, by M. Becquerel and myself, by putting together the force which presides over the circulation of *chara* and that of voltaic electricity, have even tended to remove to a greater distance the idea that this circulation was due to electricity. The latter force, besides, from what we know of it, is altogether different to the singular properties which have presented themselves to me in the force which presides in the circulation of *chara*. I have shown, in fact,* that the circulation on being arrested by the influence of certain mechanical causes, is re-established spontaneously, without the continued influence of the cause which arrested it; there is then here a phenomenon of *habitual* movement. The force which presides over the circulation, at first overcome or suppressed, has the property of *re-acting* spontaneously to establish the arrested circulation, and that after a suspension of a greater or less duration. Now, the electrical forces, such as they are known to us, do not offer anything similar to this property of *reaction* and *habit*.

2. Nothing is less philosophic, in the researches for the causes of phenomena, than always to endeavour to refer the effects observed to causes which are known to us, as if our feeble knowledge embraced all the causes of natural phenomena. This disposition of the mind is, in fact, the very circumstance which removes us farther out of the way of making discoveries. I have thought it my duty, in many circumstances, to guard especially against this, and more particularly in the study of the phenomena which are in action here. The cause of the circulatory movement of *chara* escapes all direct

* Observations on the circulation of fluids amongst the *chara fragilis*; in the *Annales des Sciences Naturelles*; January, 1838.

research ; the moving forces which are known to us do not explain it. I have sought to discover if there was not in physics some phenomena of movement, the cause of which was equally unknown, and which appeared to offer some analogy to the movement which is observed amongst chara. In this comparative examination, a physical phenomenon, very commonly known, presented itself to my mind. I allude to the movement which parcels of camphor present when placed on the surface of water ; these parcels, by the very force which moves them on the water, gives a movement to the liquid if they are fixed in such a manner as to remain immoveable. May it not be possible that the moving force which animated them, is equally the same as that which animates the green globules fixed on the sides or interior partitions of the central tube of the chara, from which green globules evidently emanates the moving power to which is due the circulation of the liquid which touches them, and likewise the inert corpuscles which this liquid carries ? I resolved to verify this suspicion by research, by leaving completely aside all the explications which had been given touching the movements of camphor on water, of which explications I felt the entire insufficiency. This latter phenomenon was to me, like that to which I compared it, an inexplicable phenomenon, and in order to know if the two phenomena had really any analogy between them, I resolved to submit the movements of camphor on water to proofs analogous to those to which I had submitted the circulatory movement of water, that I might be enabled to see whether the results would be the same. This is what I have executed, descending thus from physiology to physics ; and taking from the first of these sciences the principles of experimenting, which I transported into the second, I have had the satisfaction of seeing my expectations confirmed. I shall not follow, in the exposition of my experiments, the order in which they have been made. The new facts which were successively revealed to me, and that often with much difficulty, by these numerous experiments were not those which ought to occupy the commencement of the series in which they ought to be naturally placed : it will then be this natural series of facts to which I shall confine myself. I shall commence by studying the movement of camphor on water, in order to acquire the knowledge of the true cause of it, afterwards, taking successively the different experiments which I have made on the circulatory movement of chara, I shall attempt to apply them by imitation, to the analogous experiments made on the movement of camphor. If in these comparative experiments I obtain similar results, I shall have a foundation to conclude therefrom that the physiological force which produces the circulatory movement of chara, and the physical force which produces the movement of camphor on the surface of water, are identical.

3. The discovery of the movements of camphor on water belongs to Romieu,* who attributed these movements to electricity, by esta-

* Mémoires de l' Academie des Sciences, 1756.

blishing himself in support of this assertion on erroneous observations. He pretended that if the water on which was floating the parcels of camphor is contained in a metallic vase, we should not perceive any movement in them, whilst the movement would be manifested if the vase is of glass, of sulphur, or of rosin; this is not true. Camphor moves on water placed in vases of any kind of material whatever.

4. Benédicte Prévost* rejecting the idea of explaining this phenomenon by electricity, attributed the movements of camphor on water, to an atmosphere of the fluid, odorous and elastic, emanating from the camphor. This effluvia meeting with resistance from the air around it, and likewise from the water, reacted mechanically on the parcel of camphor, and thus impressed it with motion. B. Prévost did not limit his observations to camphor; he extended them to all odorous substances, and he found that parcels of cork soaked in these liquid substances, moved on water precisely in the same manner as camphor. At a later period he announced that camphor, when placed on the surface of mercury, presented the same movements as when placed on the surface of water.

5. Fourcroy, the author of the extract from the memoir of B. Prévost, published in the *Annales de Chimie*, adds, at the end of this extract, that he thinks the movements of camphor on water may be referred to the attraction of the odorous matter for the air and water, and to the dissolution which is produced in one or the other, or in the two at the same time.

6. Almost at the same time, Venturi† published his researches on the same phenomenon; he admits that camphor, when placed on water, emits an oily vapour, which unites itself with the superficial parts of this liquid, and that the movements of the pieces of camphor is only the mechanical effect of the reaction of this oil in spreading on the water, exercised against the camphor itself. He saw that sawdust soaked in oil turned on water in the same manner as camphor. B. Prévost observed that camphor evaporated 30 or 40 times quicker when placed on water, than when simply in contact with the air. Venturi confirmed this fact by the following experiment. Having cut pieces of camphor into columns of diminutive size, he plunged them vertically into water, leaving their upper parts above the surface of the water. These columns of camphor underwent an evaporation far more rapid at the parts where they left the water than in the remainder of their extent, which was situated in the air, in such a manner that they were not long in being cut asunder in that place.

7. Carrodor‡ reviewed the experiments of B. Prévost and of Venturi; he admits with the latter, that camphor owes its movements to the expansion of an oil which emanates from this substance,

* *Annales de Chimie*, T. 21, N 40.

† *Annales de Chimie*, T. 21.

‡ *Mémoires de la Société Italienne*, T. 11, 12, and 15.—*Annales de Chimie*, T. 37 and 48.

and which, attracted by the surface of the water, on which it becomes rapidly extended in an extremely thin layer or lamina, it repulses mechanically all the light bodies which float upon this liquid, and consequently repels the piece of camphor itself. This *attraction of surface* is exercised in the same manner by water on the fixed or volatile oils, and that by preference to the oil which is reputed to emanate from the camphor, in such a manner that the movement of the latter finds itself suddenly arrested by the projection on the water of a single drop of one of these oils.

8. Serullas* having discovered that potassium allied to different metals, and placed in small fragments on the water, presented movements altogether similar to those of camphor, sought for the cause of these movements. The potassium by decomposing the water, seizes on its oxygen, and disengages the hydrogen in the form of gas. Now Serullas having observed that the movements of fragments of alloy of potassium on the surface of water, or even when submerged in water, always took place in a contrary direction to the point where he found the hydrogen most freely liberated, concluded therefrom that it was the effluvium of this gas which, encountering resistance, either from the water, or on the part of the air, reacted on the fragment of alloy, and thus impressed the movement upon it. Analogy would lead us to admit that it was in the same manner, by an effluvium from its own substance, that the camphor struck or acted upon the water or the surrounding air, and that this was the cause of its movement on the surface of water; it is in fact a reproduction of the theory of B. Prévost.

9. M. Matteuci† admits that it is entirely to the evaporation of the camphor, and its solution in the laminae of water which encircle it, that we ought to attribute the cause of its movement. He has made many experiments in this branch, from which he believes that he may conclude that *it is to the currents of the vapours of volatile substances that their rotation is due.*

10. We remark that in the experiments made by the philosophers whom we have just quoted, all the bodies susceptible of moving themselves on the surface of water are accounted as having no power to produce this movement except by the means of an emission of a vapour or a gas, according to the opinion of B. Prévost, Serullas, and Matteuci; or by means of an expansion of their substance on water, in virtue of *the attraction of surface* produced by this liquid, as Carrodori views it. Now, experiment has proved to us that this phenomenon of spontaneous movement may be observed in many circumstances where there is no production of vapour or gas, nor any *attraction of surface* on the part of the water. I have found that caustic soda and potash sustained on the surface of the water by pieces of cork, soaked in their solution and dried, moved with a great rapidity; parcels of soap presented the same phenomenon. Soap made from grease and soda possessed this property

* Journal de Physique, T. 91, P. 172.

† Annales de Chimie et de Physique, T. 53, page 216.

more particularly; its movements on the surface of water were of great duration, and far longer than those of soap made from oil and soda. I noticed even here a remarkable fact, because it tends to establish the analogy of the cause to which the movement amongst camphor and soap likewise is due. It often happens that a piece of camphor in undergoing these movements on water is suddenly divided into two pieces, which repel each other with great activity at the moment of their separation; now the pieces of soap which move on the surface of water often present the same phenomenon. Camphor makes its movements by sudden jerks or intermissions; but it is not so with soap, and I may even say here that these sudden jerks are a general character of this phenomenon of movements in all the substances which present it. The acids and the neutral salts possess too, but in a more feeble degree, the property of spontaneous movement on water when they are held in suspension. Pieces of cork also, very slightly soaked in concentrated sulphuric acid, move slowly on the water by sudden jerks; if the pieces of cork were completely soaked in the acid, they would be precipitated to the bottom of the vessel. Pieces of cork soaked in a solution of tartaric acid or oxalic acid, and afterwards dried, present the same phenomenon of feeble movement on water. The neutral salts, prepared in the same manner, also present this movement on water, but with still greater feebleness. In general it is the substances which have the greatest affinity for water which are apt to present this phenomenon of motion. Thus the chloride of calcium moves spontaneously on water, whilst the chloride of sodium or marine salt does not move in a sensible manner.

11. The theory of these movements may be established, at least in an empirical manner, by observation of what passes when we make a solution in water of a solid body which is very soluble; of a small morsel of soda, or of caustic potash, for example. By observing with a microscope what takes place during this rapid solution, we see the small fragment of alkali immediately become the centre of a very active centrifugal movement; it projects from all its parts the dissolved substance in the surrounding water, as if it repulsed its solution. Now an observation reported above proves that this repulsion exists effectively. A piece of cork, soaked in an alkaline solution and then dried, being placed on the surface of the water under the microscope, we see the alkaline solution project itself actively in the water, and the piece of cork, unequally impregnated with the alkali in its circumference, moves in the opposite direction to that in which is going on the most active projection of this same solution. This movement of the piece of cork is evidently an effect of recoil produced by the repulsion which the solid alkali that it contains exercises on its own solution. It is very probable that this repulsion is electric, and proceeds from the fact that the solid body which is dissolved possesses a similiar electricity to that possessed by the solution. But from whatever cause it proceeds, the fact of the reciprocal repulsion of soluble bodies and their aqueous solutions

is certain, and it is to this repulsion that we may attribute the movement which takes to the surface of the water all the floating bodies, which are there dissolved ; this is what is observed not only in the alkalies, the acids, and the salts, but also in certain gum-resinous substances, such as opium, succotrine aloes, &c. We now come to the movement which takes place of oily or resinous bodies on water.

12. A drop of the essential oil of turpentine or of lavender, thrown on the surface of the water, spreads itself rapidly there, in a lamina so thin, that it reflects the colours of the iris ; a second drop does not produce the same effect. The fixed oils present a similar phenomenon when they are not too viscous. Thus, for example, the oil of wild cabbage, which is very viscous, does not spread on the surface of water ; but when it is rendered more fluid by *purification*, which renders it fit for being used for lighting, it spreads itself very well on the surface of the water. This phenomenon does not always take place with the first drop ; the second drop preserves its globular form at the surface of the water. Having one of these gross drops of fixed oil floating in the water, I have deposited gently in its centre a drop of the essential oil of turpentine ; this gradually pierced through the thickness of the drop of fixed oil, as if it was attracted by the subjacent water, and at the moment of its contact with the water it made a sort of explosion, by projecting circularly its substance on the surface of the water ; chasing before it in a circular movement the fixed oil which imprisoned it, and which after this explosion, returned on itself, so as to form itself anew into a drop. The cause of this centrifugal movement is exactly the same as that which has been determined above with relation to soluble bodies. The solution, considered in its latter limits, is, in fact, only the reunion of molecule with molecule of the liquid dissolving and of the body dissolved. We then have the penetration of the dissolving liquid until this end is obtained. But if the dissolving liquid and the body to be dissolved are not miscible, then there will be no penetration of liquid, and the solution only operates at its surface. This is what takes place with oils brought into contact with water. The drop of oil, which by a centrifugal movement, extends itself on the water in a thin and irised lamina, here presents, in fact, the phenomenon of the solution considered in its latter limits, that is to say, the reunion, side by side, of each molecule of water at the surface, with the molecule of oil. In the action of this superficial and non-penetrating solution, we observe, as in all the solutions which have the property of penetration, a movement of repulsive projection, parting centrally from the body which is being dissolved, by projecting around it its own substance. Here it is each molecule of oil which repels the neighbouring ones, in such a manner that a circular projection is the result of this assemblage of repulsions. If a second drop of fixed oil projected on the water does not produce the same phenomenon as the first, it is because it has saturated the surface of the water ; but this saturation of fixed oil does not hinder the subsequent solution of a drop of essential oil. The oil, by thus

rapidly invading the surface of the water, mechanically chases before it the light bodies which float on the surface of this liquid, as we have just seen that the drop of essential oil chases around it the drop of gross fixed oil which imprisoned it. This propulsion of floating bodies is quite mechanical; it is not *repulsion* in the sense generally attached to that word; the true repulsion exist here only between the molecules of the oil. These observations directly conduct us to the study of the movements of camphor on water.

13. Camphor is an essential oil concreted; it ought then to offer the same phenomena as the essential oils upon its contact with water, with this difference, always: that it is not in the state of an *oily liquid* that it unites at the surface of the water, but much in the state of *oily vapour*, which is in reality the same thing. This manner of viewing this phenomenon conciliates the opinions of Benedict Prévost and of Carrodori, whose opinions represent, each in part, a part of the truth. The oily vapour of camphor becoming united to the surface of the water and undergoing an active molecular repulsion in the action of this superficial solution, extends itself rapidly on the surface of these liquids, by chasing before it all the light and floating bodies which it there encounters. This apparent repulsion, which is in fact only a propulsion, is often extended to a distance of more than thirty millimetres around the piece of camphor. If we project on the water a drop of essential or fixed oil, this oil invades the surface of the water in preference to the vapour of camphor, and the latter instantly ceases its movement. We here have a phenomenon of elective affinity. The surface of the water becomes saturated with the fixed or essential oil, and from that time is no longer susceptible of dissolving the vapour of camphor; it is sufficient even that the air be charged with the odour of an essential oil, or of any odour whatever, to present an obstacle to the movement of camphor or water; and that because of the odorous vapour being dissolved on the surface of water in preference to the vapour of camphor. When the air is highly charged with the vapour of camphor, the movement of the latter on water is equally arrested, and that because the surface of the water saturates itself completely with this vapour. We now see why the movement of camphor is arrested when the vase which contains the water in which it moves is covered. It is necessary that the vapour of camphor dissolved by the surface of the water be able to evaporate freely and promptly, in order to give up its place to a new dissolution.

14. When it happens that a small fragment of camphor, which is moving on the surface of the water, becomes divided into two parcels, these two pieces repel each other with extreme vivacity at the moment of their separation. This effect is due to the expansion, or to the molecular repulsion of the vapour of camphor interposed between these two pieces. We can perfectly conceive, that in the expansion of the vapour of camphor at the surface of the water, it meets with a resistance on the part of this liquid which ought to

induce a reaction against the piece of camphor, and the latter ought consequently to move by the effect of recoil. Now, it is not on this cause alone that the movement of camphor placed on the surface of water depends; the most powerful cause, and the most important in consideration in the production of this phenomenon has hitherto escaped observation. We now proceed to observe the developement of physical phenomena whose existence has been far beyond our suspicion. I shall now resort to experiment.

15. I put a small quantity of water in a watch glass; it amounted to two or three millimetres in height, and I placed on this water a piece of camphor less than half a millimeter in diameter. This piece of camphor, confined in a small space, was not long in gaining the side of the water, and it there remained fixed, agitated solely by a trembling movement. I submitted it to the microscope, employing the feeble magnifying power of ten times its diameter, which enabled me to perceive a field of seven or eight millimetres of diametrical extent. I then added to the water a drop of muddy water, holding particles of white clay in suspension. Instantly a curious phenomenon was presented to my observation. The particles of the clay were observed to be precipitated towards the piece of camphor; the current which they formed, having arrived in the neighbourhood of the camphor, then divided into two currents, one of which was directed towards the right and the other towards the left; that is to say, towards the two opposite extremities of the piece of camphor; there they undergo a sudden repulsion, and retreat from each other with a quickness which gradually diminishes. On arriving at three or four millimetres distance from each other, they describe a curve which brings them back into the flowing current, and they are, now, again precipitated towards the camphor which attracts them, and impresses on them an accelerated movement. There is thus established two vortices, directed in inverse directions, and in each of which the terreous particles in suspension undergo a real circulation which operates in an ovoïde curve, the smaller end of which is near the camphor, and the larger end at that part opposed to this curve. Thus the two actions of attraction and of repulsion which the terreous particles undergo are not exercised in right lines, but following the two parts opposed to an ovoïde curve more or less elongated. We observe that the repulsion takes place at a small distance from the camphor, in such a manner that the particles of clay are repulsed by it without having touched it. This repulsion takes place at a distance from the camphor which I value as $\frac{1}{10}$ of a millimetre, and sometimes at $\frac{1}{20}$ or $\frac{1}{30}$ of a millimetre at most.

16. I have rendered these phenomena still more easy to be observed by replacing the terreous particles in suspension by the yellow and flocky precipitate which is formed in the solution of sulphate of iron in water. In that case, in place of adding, as in the preceding case, a drop of muddy water to that contained in the watch glass, I here add a drop of the solution of sulphate of iron, highly

charged with the flocky precipitate of which I have just spoken. The fleeces of this precipitate, by reason of their size, are more easily observed than the particles of clay. It happens sometimes that these flocks suspended in water, in place of describing in their movement the ovoïde curves of which I have above spoken, rest near the piece of camphor, and there, at $\frac{1}{10}$ of a millimetre distant from this piece, they present a rapid rotatory movement on themselves, and on a horizontal axis, sometimes in one direction and sometimes in an opposite one. There is here, however, no change in the direction of the rotation; when that is established in one direction it persists in it. There exists then at one or two tenths of a millimetre around the particle of camphor a rotatory moving force, the direction of which is not the same at all points of its periphery; the corpuscles floating in the water, and which are attracted by the camphor, all receiving on arriving near it, this rotating movement on themselves, whilst at the same time they receive the movement of repulsion. They promptly lose this rotating movement on retreating from it, and that by the effect of the resistance which the water subjects them to by reason of their irregularity of form; they would probably retain this rotatory movement during their revolution, if their form was perfectly spherical. As for the rest, it is evident that the rotation of the corpuscles themselves and their revolution in a limited curve, are two modifications of the same phenomenon. In fact, the two moities of the revolution, the first of which is due to attraction and the second to repulsion, are, in relation to each other, just the same as the two moities of the rotation are to each other: of which the first, which is analogous to that moiety of the revolution due to attraction, commences at that part of the turning body actually opposed to the camphor, and finished in regard to the latter; and the second, analogous to that moiety of the revolution due to repulsion, commences with regard to the camphor, and finishes at the opposite place.

17. It results from these observations that the movement of rotation is here a modification of the movement of revolution. The cause of these two movements is the same. Another result, also, is that the attraction and repulsion are here the two opposite directions of the same force, which are exercised according to a limited or closed curve,—a curve whose greater axis corresponds by one of its extremities with the neighbourhood of the body in which is found the origin of this force.

18. Hitherto I have not studied the movements which the camphor impinges on the particles suspended in the water, and that thus it only acts on this liquid upon one of its sides. It was important to observe what happened when the camphor is situated on the surface of the water which environs it circularly. It is not easy to make this observation with the microscope, because the piece of camphor placed on water is generally animated by a progressive movement. But it often happens that this progressive movement, after having lasted for some time, ceases; the piece of camphor may

then rest in its place in the middle of the water, being agitated solely by a slight trembling movement. We may then easily observe by the microscope the effects which it produces. We do not see in this case the two vortices, with opposite directions, which are constantly observed in the movements of light bodies suspended in the water, when the camphor is fixed at the edge of the liquid; we then see these light bodies precipitated equally on all sides on the piece of trembling camphor, and they there receive also the movement of repulsion which always takes place in a different direction to that in which is produced their movement of attraction, in such a manner that in returning incessantly towards the camphor, from which they afterwards retreat, they describe limited and ovoïde curves which are extremely multiplied, and which appear to cross each other in all directions. I have also observed, in this case, the movement of rotation of large flocks situated at a very small distance from the camphor. I put water charged with argillous particles in suspension in a saucer, and at the height of some millimetres, I suffered the argillous particles to be precipitated to the bottom of the water; I then put a piece of camphor on the surface of this liquid, now become limpid. On observing it through a magnifier, I saw the pulverated argillous sediment raised from the bottom of the vessel, especially in those parts where the piece of camphor was transported to, and when the latter remained in one place by turning on itself, the argillous sediment being raised by its attraction turned with it, thus forming a sort of small truncated waterspout whose base is at the bottom of the vessel.

19. On the inspection of these phenomena, no one will doubt that they are due to electricity; but we may be permitted to ask, if these same phenomena are explicable by means of any facts known to science. The attraction and repulsion of light bodies only belongs to static electricity, again they produce this effect in the air only, and not at all in water. Some phenomena of rotation are produced under several circumstances by electro-magnetism; but in the experiments here under consideration, the phenomena of rotation takes place without the intervention of magnetism. I ought, however, to abstain from pronouncing on this question, and I will now pursue my observations.

20. The actions of attraction and repulsion which camphor exercises on light bodies floating in water, are here the indices that these same actions are exercised on the water itself; they are, then, the principal causes of movement which are presented by camphor; it attracts and repulses alternately the water which surrounds it; it ought to be moved then, at the same time as the water, and even more than it, since it is more moveable. It is from the rapid and continual succession of these two opposite actions that the trembling movement presented by camphor results; its turning ought to be attributed according to my observations, to the fact that the piece of camphor, when turning, possesses a lateral point, on which the electric repulsive effluvia especially operates; and which, by reaction

makes the piece of camphor turn on itself by the same mechanism which causes the rotary movement in a pyrotechnic sunflower. The sudden and jerking, and I may say too, almost capricious movement which the piece of camphor presents on the water, in all sorts of directions, is the result sometimes of reciprocal repulsion which exists between it and the water, and sometimes of their reciprocal attractions, according to the accidental predomination of one or the other of these two actions. To this cause of movement is joined the effect of recoil which results from the repulsion which the piece of camphor exercises on the superficial solution, as it has been already explained in this paper. I have shown how this superficial solution is repulsed by the piece of camphor; likewise, that all the solutions are repelled by the soluble bodies from which they emanate. Now, as it is proved here that the piece of camphor possesses an electric state, a source of attractions and repulsions, it ought no longer to appear doubtful that the repulsion which it exercises on its superficial solution is due to electricity; besides this, it is this electricity which is the cause of the more rapid evaporation of camphor when it is placed in water than when it is placed on another body, as Benedict Prévost and Venture have shown by experiment. This electricity, and the effects which are derived from it, are observed when even in contact with all the oily, greasy, or resinous bodies, when these bodies can undergo a solution, whether *penetrating* or superficial on this liquid. Thus, the particles of soap brought into contact with water charged with argillous particles in suspension, and placed under the microscope, present exactly the same electric phenomenon, and even with much more intensity; for the effects of attraction and repulsion on the argillous particles are extended much farther in the interior of the water; the field of the most feeble microscope cannot then show, in all their extent, the vortices or whirlpools which are produced under these circumstances. It is remarkable, that the rapid and concentric *afflux* of the water, and the argillous particles which it holds in suspension, towards the piece of soap, commences at the instant even of the contact of the latter with the water; the phenomenon of repulsion succeeds to the phenomenon of attraction and is thus only secondary. I have observed these same electric phenomena of attraction and repulsion to be successive, and forming thus whirlpools in the water, by bringing into contact with this liquid pieces of cork soaked in essential oil of turpentine or alcohol; but these phenomena had only a very short duration, in consequence of the rapidity with which the solution was then accomplished. Also, all the oily bodies, or more generally, all combustible bodies, susceptible of being united with water by *penetrating* or *superficial* solution, take, from the time of their contact with water, an electrical state which is the cause of the repulsion which they exercise on their own solutions, which thus possesses the same electricity, or is in the same electrical state as themselves. It is by virtue of this same state of electricity that combustible bodies exercise on the water and on the solid particles which it holds in

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suspension, those attractions and repulsions which, by their succession, form whirlpools or vortices.

21. It remains for us to know for a certainty whether the alkalies, the acids, and the salts, which also possess the property of moving on water, because they repel their solutions, also present the whirlpools resulting from successive attractions and repulsions exercised on the surrounding water, and on the light bodies which they hold in suspension. As the alkalies present in a far higher degree than the acids and salts the property of moving on the water, it will be on them that I shall here found my observations. I have already explained above, the experiment that showed me the fact, that a small fragment of caustic soda immersed, under the microscope, in water charged with argillous particles, presents the centrifugal movement of its solution, which flies from it on every side. I have made this experiment with spring water. Now, on the solution of soda in this water, there was a large number of bubbles of gas disengaged, which, like the solution, obeyed the centrifugal movement, and which were an obstacle to the distinct vision of that which was taking place around the small fragment of soda in course of being dissolved. In order to avoid this inconvenience, I made use of water which had been deprived of air and carbonic acid by ebullition. I put into it the argillous particles in suspension. A small fragment of caustic soda was placed in this water, under the microscope, with a magnifier of ten times its diameter. I then discovered, by means of the absence of the bubbles of gas, another phenomenon besides that of the centrifugal movement of the solution, which was the only one I had perceived previous to that. I saw all around the small fragment of soda whirlpools, indicated by the alternate movements of attraction and repulsion of the argillous particles, which thus moved in short curves, apparently circular, and whose diameter did not exceed three-tenths of a millimeter. This was the same phenomenon, but about three times smaller, as that which camphor on the surface of water had presented to me (15); and likewise the progressive movements which an alkali presented at the surface of water, when it is held in suspension by pieces of cork, are due exactly to the same causes as those which produce the movements of camphor: namely, the repulsion exercised by the bodies which are dissolved on the aqueous solution, and the alternate actions of attraction and repulsion exercised by this same body on the water which surrounds it. We cannot doubt but that the feeble movements which certain acids and salts present, when held in suspension on the surface of water, are due to the same causes. I may say the same of the movements presented in the same circumstances by small fragments of aloes succotrine, opium, &c. In all cases, it is the action of the solution which gives birth to these electrical phenomena, and which manifest themselves by attractions and repulsions on the surrounding liquid, and the effect of which reaction puts in movement these soluble bodies where they are floating.

22. The vigorous movement of progression which potassium pre-

sents when burning on the surface of water, is a phenomenon due, like the preceding ones, to an electric action, and not to the rapid disengagement of hydrogen gas, as we have already admitted. To be assured of this fact, it is not necessary to have pure potassium to submit here to observation, its combustion and its effects on water are too rapid; it is necessary to observe it alloyed with antimony, made after the method described by Serullas. This alloy, reduced into small fragments, moves with rapidity, whether it is floating on the surface of the water, or whether it is entirely immersed in this liquid. The potassium, thus alloyed with antimony, does not decompose the water with great rapidity, as pure potassium does; in this case, the action is not very brisk, and endures sufficiently long to enable us to study its effects. I have placed under the microscope, and with the feeble magnifying power of ten times the diameter, a small fragment of the alloy of potassium and antimony, placed on a plate of glass, at the side of a small sheet of water which held argillous particles in suspension. At the moment of contact of this fragment of alloy with the water, there was produced a disengagement of bubbles of hydrogen gas, directed like a rapid *jet* towards the interior of the small sheet of water. The direction of this *jet* was always the same, though the fragment of alloy was agitated and returned in different directions by the effect of recoil or of reaction. On arriving at five or six millimetres distance from the fragment of alloy, the *jet* of hydrogen gas disappeared; because the small bubbles which composed it issued from the water and spread themselves in the air. But the *jet* itself was not discontinued at any time, it went farther on, formed only then by the water, rendered visible by means of the argillous particles which it held in suspension. Soon, however, this *jet* became divided into two parts, turning towards the right and left, and I saw the argillous particles return, on accomplishing a revolution, towards the fragment of alloy, in order to enter anew into the *jet*, which was thus evidently at the time composed of water and bubbles of hydrogen gas. But the production of this gas soon ceased to be either so rapid or so abundant, the bubbles were disengaged slowly, and no longer quitted the sides of the small fragment of alloy. However, this *jet* continued to be evolved, though only indicated by the movement of the argillous particles; which afterwards returned, by parting towards the right and left near the fragment of alloy, thus forming two eddies or whirlpools. There was here, then, evidently alternate actions of repulsion and attraction of the water, like that which was observed from the contact of oily or resinous bodies with water; and there can be no doubt, but that these actions are the same, and that they are electrical. The *jet* here in question is the result of repulsion, which, being very active at first, acts at the same time on the water, or rather on the solution of potash in the water, and on the bubbles of hydrogen gas, which are abundantly disengaged. This gas being afterwards only slowly disengaged, and the repulsion becoming less active, it no longer acts on the water, or on the solution of potash

the movement of which solution is rendered visible by the particle, of clay which it holds in suspension. Though there be no longer then the *jet* of bubbles of hydrogen gas, the fragment of alloy does not discontinue to be agitated by an effect of recoil or reaction; it is not then the effluvium of this hydrogen gas which is the cause of this effect of recoil, as is supposed admitted by Serullas; this gas like water obeys an electric repulsion, and it is this repulsion which is the sole cause of the effect of recoil. Serullas has here taken the effect for the cause.

23. I make here an important remark to be taken into consideration. Upon the contact of camphor or of oily bodies with water, electric attraction is the primordial effect, repulsion is the secondary effect: whilst this is the inverse of what takes place on the contact of the alloy of potassium and antimony with water. Thus, a piece of camphor being placed under the microscope, at the brink of a small sheet of water on a plate of glass, and holding particles of clay in suspension, we see at the moment, that the water charged with argillous particles is precipitated, by the effect of attraction, towards the middle of the piece of camphor, and then divided into two currents, the one turning to the right and the other towards the left, which receive the repulsive action and retreat, by each accomplishing a revolution, in order to return and join themselves in the unique and medium current due to attraction. Now a piece of the alloy of potassium with antimony being placed similarly in this experiment, the effect which takes place will be inverse. We see, at the moment of contact, the water, or rather the alkaline solution, charged with argillous particles and mixed with bubbles of hydrogen, shoot forward from the middle of the piece of alloy by the effect of repulsion, and become divided afterwards into two currents, the one right and the other left, which then obeying the attraction, return towards the piece of alloy by thus accomplishing a revolution, in order to unite itself anew to the unique and medium current due to the repulsive action. Thus on the contact with water of combustible, oily or resinous bodies, susceptible of uniting themselves with this liquid, we observe an *afflux*, by the attraction of the water as the primordial phenomenon; the subsequent repulsion of this liquid is the secondary phenomenon. On the contact of water and potassium, which is a combustible body of another kind, it is the repulsion of the water which is the primordial phenomenon, its subsequent attraction is the secondary phenomenon. Now it is not the potassium which becomes dissolved in water, it is the potash which forms by the combination of the oxygen of the water with the potassium. Thus it is to the solution of potash that we must attribute the electrical phenomena which manifest themselves in the contact of the alloy of potassium and antimony with water. We have already seen (11) that these electrical phenomena manifest themselves in the same way, but in a less apparent manner, on the immediate solution of a caustic alkali in water. The spontaneous movements which are manifested in a manner more or less apparent,

by all other bodies soluble in water, when they are kept artificially floating at the surface of this liquid, prove that they equally develop under these circumstances, an electricity which is the cause of these spontaneous movements. I shall designate these electromotive phenomena due to the action of solution, by the name of *diluo-electric phenomena*. Now it results from some of the observations which have just been detailed, that two diluo-electric phenomena, different up to a certain point, since they produce two rotations in inverse directions, are produced by the solution of combustible bodies in water, and by the solution in the same liquid of bodies that are non-combustible. We feel then the necessity of here distinguishing the two orders of *diluo-electric* phenomena. As I shall not have to employ myself in this memoir with the *diluo-electric phenomena* produced by the solution of non-combustible bodies in water, I need not impose upon them a particular name. It will be with the *diluo-electric* phenomena, produced by the *penetrating* or *superficial* solution of bodies combustible in water, that I shall occupy myself exclusively; and I shall designate them by the special name of *camphoro-electric* phenomena; and that because it was camphor which first presented these phenomena to observation, and it is camphor likewise, under this point of view, which is in some manner the type of all the bodies which are susceptible of presenting these same phenomena. I shall give also to all those combustible bodies, considered under the point of view of special electric phenomena which they are susceptible of producing, the general name of *camphoroidal* bodies.

24. I have previously observed (10) that one of the characteristics of the movement of camphoroidal bodies on water, was the suddenness of the accidental and intermitting acceleration of this movement which present from time to time the appearance of lively jerks. In observing, by a microscope, the movement of the argillous particles in suspension in the water which was in contact with a piece of soap, I have seen at the moment of these sudden jerks, that the argillous particles, and consequently the water which held them in suspension, suddenly underwent an impetuous repulsion. We cannot then doubt that these sudden jerks proceeded from a sudden discharge of electricity in the proper manner to produce the repulsion of the water, from whence it follows that the piece of soap was suddenly moved in an opposite direction by the effect of recoil.

25. It is not only at the surface of the water that these camphoroidal bodies can move; they have even been observed to move, under certain circumstances, in the middle of the water. Thus the alcohol, which is imbibed by pieces of cork, gives a very active movement to those pieces placed on the surface of the water; now when this same liquid is imbibed by small fragments of organic matters whose specific gravity causes them to penetrate into the interior of the water, it then gives a very vigorous movement of progression to these small fragments, and the movement takes place, as ordinarily, by sudden jerks. We must remark, that this electricity which has

birth from the contact and of the reciprocal solution of alcohol and of water, does not manifest itself as a moving cause except in the alcohol, and not at all in the water. Thus the small pieces of organic substances soaked in water do not move at all spontaneously when they are put into alcohol.

26. The *diluo-electric* phenomena, of which the *camphoro-electrics* form a division, seem to hold the middle between the phenomena of static electricity and those of dynamic electricity. They belong to this latter by their origin, which is taken from a chemical reaction; they present like the former, successive attractions and repulsions to the bodies which surround the electrised body, which is not produced by dynamic electricity. Does the electricity form in any manner a part? I abstain from giving any opinion in this respect. Taken altogether, the numerous experiments go to show that this electricity possesses special properties, and properties which appear strangers to static electricity as well as to dynamic electricity. These properties are in relation, first, with the nature of the material of the vase which contains the liquid on which the camphor moves, or more generally, the *camphoroidal body*, and with certain physical states in which the vessel may be found; secondly, with the chemical nature of the aqueous liquids on which the *camphoroidal bodies* are susceptible of motion, and with certain physical states of these same liquids. It was very necessary that I should run in its entire extent the vast field of research which embraces the novel order of facts which I have made known: but I have only, in this respect, merely opened the way. Camphor especially, and almost exclusively, served me in my experiments; I have only made a very small number with soap, which is a *camphoroidal body*. The aqueous liquids of which I have made use have been pure water, different alkaline, acid, or saline, solutions. Glass vessels are those of which I have especially made use; I made a great number of experiments, however, in which I employed metallic vessels, vessels of baked earth, of wood either in its natural state or varnished, of wax, &c. I submitted successively these different experiments, the principal results of which I have deposed in a *pacquet* under a sealed envelope, which the Academy accepted at its session of the 1st November last.

On the Perchlorate of the Oxide of Ethule, or Perchloric Ether.—

By CLARK HARE and MARTIN H. BOYE.*

THE energetic properties of perchloric acid, and its stability, compared with the other compounds of chlorine with oxygen, led us to the belief that this acid might be combined with the substance which performs the part of a base in that class of organic salts which are generally designated by the name of ethers, and for which Berzelius, in consequence of his theoretical views, has adopted the name of oxide of ethule. For this purpose a concentrated solution of per-

* Read before the *American Philosophical Society*, Dec. 4, 1840.

chlorate and sulphovinate of barytes, in equivalent proportions, was subjected to distillation. The sulphovinate of barytes may be considered as a double sulphate of barytes and the oxide of ethule; and we anticipated that when heat was applied a double decomposition would take place between the latter and the perchlorate of barytes. So long as the salts remained in solution, no reaction occurred, but as soon as they became solid, in consequence of the distillation of the water, a reciprocal decomposition ensued, and a sweet ethereal liquid distilled into the receiver. This liquid is the perchlorate of ethule.

As this substance is extremely explosive, it is necessary, in order to prepare it with safety, to operate on small quantities. We have employed from seventy to ninety grains of crystallised sulphovinate of barytes, with an equivalent proportion of perchlorate of barytes;* but we would recommend, especially on the first performance of the experiment, the employment of considerably smaller quantities. The salts should be intimately mixed in a mortar, and placed in a small retort attached to a refrigerator containing ice, and a receiver similarly cooled. The retort is to be heated in an oil bath, in which a thermometer is suspended, so as to indicate the temperature. A wooden screen, furnished with openings covered with thick plate glass, at such intervals as to afford a full view of the different parts of the apparatus, should be erected in front of it, and strings passed around the screen and attached to a bar traversing on a pivot, and supporting an argand spirit lamp, by which heat is communicated to the oil bath, so as to enable the flame of the lamp to be removed from or applied to the apparatus, according to the indications of the thermometer, without exposing the person of the operator. After the heat has reached 212° F., below which the salts employed do not react on each other, it should be raised very gradually, and the distillation finished below 340° F. Under these circumstances but little danger is to be apprehended from the retort, but the ether in the receiver must be treated with the greatest caution, since it has exploded in our hands in attempting to remove it with a pipette from the stratum of water which covers it. This water, therefore should be removed by the cautious use of strips of blotting paper, moistened at the end, and introduced into the tube employed as a receiver.

To avoid the danger attendant on the management of the ether in its pure state, it may be received in strong alcohol, since it is not explosive when dissolved in alcohol. If the experiment be performed with seventy grains of sulphate of barytes, from one to two

* The amount of barytes in the perchlorate should be ascertained by an experiment, as it retains water with great tenacity. It may be worth while to mention, that the perchlorate of potassa cannot be substituted for the perchlorate of barytes, since the sulphovinate is decomposed without acting on it. We were equally unsuccessful in an attempt to procure the ether by the distillation of perchlorate of barytes and concentrated sulphovinic acid.

drachms of absolute alcohol will be found sufficient for this purpose. By the addition of an equal volume of water the ether may subsequently be separated from this solution in small quantities, for the purpose of examination. But in this case, a loss of ether is sustained by the decomposing influence of the water employed.

The perchlorate of ethule obtained in this way is a transparent, colourless, liquid, possessing a peculiar, though agreeable smell, and a very sweet taste, which on subsiding leaves a biting impression on the tongue, resembling that of the oil of cinnamon. It is heavier than water through which it rapidly sinks. It explodes by ignition, friction, or percussion, and sometimes without any assignable cause. Its explosive properties may be shown, with but little danger, by pouring a small portion of the alcoholic solution into a porcelain capsule, and adding an equal volume of water. The ether will collect in a drop at the bottom, and may be subsequently separated by pouring off the greater part of the water, and throwing the rest on a moistened filtre, supported by a wire. After the water has drained off, the drop of ether remaining at the bottom of the filtre may be exploded, either by approaching it to an ignited body, or by the blow of a hammer. We are induced to believe that in explosive violence it is not surpassed by any substance known in chemistry. By the explosion of the smallest drop an open porcelain plate will be broken into fragments, and by that of a larger quantity be reduced to powder. In consequence of the force with which it projects the minute fragments of any containing vessel in which it explodes, it is necessary that the operator should wear gloves, and a close mask, furnished with thick glass plates at the apertures for the eyes, and perform his manipulations with the intervention of a moveable wooden screen.*

In common with other ethers, the perchlorate of ethule is insoluble in water, but soluble in alcohol; and its solution in the latter, when sufficiently dilute, burns entirely away without explosion. It may be kept for a length of time unchanged, even when in contact with water; but the addition of this fluid, when employed to precipitate it from its alcoholic solution, causes it to be partially decomposed. Potassa, dissolved in alcohol, and added to the alcoholic solution, produces immediately an abundant precipitate of the perchlorate of that base, and when added in sufficient quantity, decomposes the ether entirely. It would appear, therefore, impracticable, to form either perchlorovينات or perchlorovinic acid.

We have subjected the perchlorate of ethule to the heat of boiling water without explosion or ebullition.

It may be observed that this is the first ether formed by the combination of an inorganic acid containing more than three atoms of

* Having suffered severely on several occasions from the unexpected explosion of this substance, we would earnestly recommend the operator not to neglect the precautions mentioned above.

oxygen with the oxide of ethule, and that the chlorine and oxygen in the whole compound are just sufficient to form chlorohydric acid, water, and carbonic oxide, with the hydrogen and carbon.

The existence of a compound of the oxide of ethule with an acid containing *seven* atoms of oxygen, led us to attempt to combine, by the same method, this base with nitric acid. For this purpose, we subjected a mixture of sulphovinate and nitrate of barytes to the same treatment as described above, but the reaction, even when conducted with the greatest possible care, is destructive; hyponitrous ether, and gaseous matters, being the principal products obtained. Nor were we more successful in our attempts to procure a sulphurous or hyposulphuric ether by the same process.

Remarks upon an Examination of the Peroxide of Manganese.

By HENRY C. LEA.*

It is of great importance, both to the practical and theoretical chemist, to have the combinations which the different metals form with the acids investigated, in order that the proper degree of confidence may be reposed in the various theories which have been formed from time to time by so many celebrated chemists. It is with this view alone that I consider these examinations as worthy of being published, as I unfortunately had not time to pursue them far enough to afford any very definite result. They may however serve to point the way to some one who has sufficient zeal for the science, to carry them out to some more useful end than I could hope to attain. I must here make my acknowledgments to Prof. Booth, in whose laboratory, and with whose assistance, these examinations were made.

To peroxide of manganese has never been investigated, as its existence has until lately been questioned by some of the first chemists in Europe, and the tendency of its salts to convert themselves into proto-salts, contributed to render it problematical whether it was not merely the protoxide disguised. It can be obtained in various ways, but the most convenient is to calcine the proto-nitrate gently until the nitrous acid ceases to be given off. A less troublesome method is to heat the common black or deutoxide, until part of its oxygen is given off, but this method is uncertain, as too great a heat converts it into the manganoso-manganic oxide, and it is almost impossible to obtain the black oxide free from admixture with iron. When obtained by calcining, its color is of a deep black, and sometimes shining; but when precipitated from a liquid, as the permanganate of potassa, it is of a dark brown. It has sometimes been found native, and is then known to mineralogists under the name of Braunite. It unites with water and forms the hydrate, which may be readily produced by precipitating the hydrated protoxide from a

* From Silliman's *American Journal*.

proto-salt, and exposing it to the action of the atmosphere. Obtained in this manner it appears under the form of a brown powder, but when found native, it is black and crystallizes sometimes in acicular crystals, and sometimes in octahedra, resembling in this state the deutoxide. The peroxide is composed, according to the calculations of Berzelius, of 43.37 of oxygen to 100 of manganese.

With the different acids it has very various actions; with some it is converted into protoxide, forming proto-salts; while with others it immediately forms per-salts, which seem to have no regular colour, some being red, while others are nearly white, brown, or yellowish; a dirty white is however the most usual appearance. I have found it to be the case, that most vegetable acids which convert the peroxide into protoxide by giving off oxygen, when acting upon the deutoxide, will form per-salts by the loss of oxygen. They all contain a very great excess of acid, without the presence of which the peroxide seems incapable of forming any salt. The best test I have met with for distinguishing them from the soluble proto-salts, to which they in appearance bear a great similarity, is the yellow prussiate of potash. With the per-salts it gives a greyish green precipitate, while with the protoxide solutions the precipitate formed is white or whitish pink. The hydrochlorate of platina is also a good test for them, as with them it forms a yellowish precipitate, but with those of the protoxide, it forms none.

Sulphuretted hydrogen.—When this gas is passed over the peroxide placed in a tube, which at the same time is heated, the gas is decomposed, sulphur and water are given off, and the oxide is converted into a sulphuret of a light green colour. The gas must be passed over until the tube becomes cool, for if the sulphuret be exposed to the air while hot, it inflames, acting the part of a pyrophorus. When digested in fuming nitric acid, a violent action takes place, the sulphuret is decomposed and converted into a proto-salt, and all the sulphur is precipitated. Analyzed in this manner it gave 9.6 per cent. of sulphur, and when heated in the open air until the sulphur was burnt out and the oxide converted into manganoso-manganic oxide, it yielded 100 per cent. of manganoso-manganic oxide, which contains 72.178 per cent. of metallic manganese. Now 9.6 of sulphur will combine with 16.51 of manganese, which makes 26.11 per cent. of sulphuret. There then remains 55.67 per cent. of manganese, which, if considered as manganoso-manganic oxide, would form an oxy-sulphuret, containing

Sulphuret of manganese.....	26.110
Manganoso-manganic oxide.....	71.893

98.003

Thus in the operation, both the oxide and sulphuretted hydrogen are decomposed. The oxide is partly reduced to manganoso-manganic oxide, and partly to metallic manganese. The sulphur from the sulphuretted hydrogen is mostly driven off, but some of it

combines with that part of the oxide which has been converted into the metal, while the oxygen from the oxide, and the hydrogen from the gas, unite and pass off under the form of steam. This oxy-sulphuret very much resembles the substance formed by gently calcining the red sulphuret in a close vessel (during which operation sulphuretted hydrogen is given off), but it upon analysis gave but 92.857 per cent. of manganoso-manganic oxide, while the first forms 100.

Cyanogen.—When cyanuret of potassium is added to a solution of a per-salt of manganese, the cyanuret is precipitated under the form of an extremely fine greyish green powder, which remains suspended in the liquid for some time.

Sulphuric acid.—The persulphate may be formed by digesting the black oxide in sulphuric acid for several days in the cold, or when peroxide is placed in dilute acid, it is formed in a few hours, but when the peroxide is used, there is a greater excess of acid. This solution is of a beautiful carmine red, but if the oxide be that precipitated from the permanganate of potassa, the solution has somewhat of a violet tinge. It has so great a tendency to convert itself into protosulphate, that it can neither be evaporated nor crystallized, and it cannot be kept for any time, as it is decomposed in the course of two or three weeks. This change may be accelerated by the addition of alcohol.

Sulphate of Manganese and Potassa.—This salt, which is the manganese-alum, may be formed, according to Mitscherlich, by adding a concentrated solution of sulphate of potassa to one of persulphate of manganese. It crystallizes of a violet brown colour, and is decomposed by the addition of water.

If bisulphate of potassa be digested upon deutoxide of manganese, there is a strong action, which results in the formation of a double salt, which, upon evaporating, remains under the form of a somewhat crystalline mass of a dirty white colour, and a pleasant acid taste; it reddens litmus paper, and shows the reaction of the peroxide with yellow prussiate of potassa, and does not seem to be decomposed by water; but it is most likely the manganese-alum of Mitscherlich.

Nitric acid.—When nitric acid is digested upon peroxide of manganese, it does not form a per-salt, but the nitrate may be made by adding nitrate of lead to the persulphate of manganese, until they are both neutralized.

Hydrochloric acid.—If this acid be digested upon per or deutoxide of manganese, there is a perchloride formed of a dark brown colour, and which decomposes immediately by the application of heat, or in a week or two, in the cold. There then remains protochloride, while chlorine is evolved. When evaporated to dryness, we obtain crystals of protochloride of a fine pink. Dr. John passed chlorine through a solution of 300 grains of protochloride, dissolved in 12oz. water, cooled to 41°. The liquid gradually congealed as

the operation proceeded, and produced a yellowish crystalline mass, which melted at a temperature a little above 41° . It was decomposed by evaporation.* This may only have been the perchloride, surrounded by liquid chlorine, for when I repeated this experiment, at a temperature above 41° , I obtained a yellowish crystalline mass, which, however, on being placed between blotting paper to dry it, proved that a yellow liquid imparted that colour to the salt, which itself was pink. I did not, however, observe that it was decomposed by evaporation.

Sulphurous acid.—This acid has no action upon the peroxide, even when passed over it in a heated tube; and with the deutoxide it forms proto-hypo-sulphate. I do not think that the per-sulphite can be formed unless by double decomposition with some other salt.

Carbonic acid.—It has no action upon the peroxide, and, as far as I have observed, it cannot be made to combine with it. The brown substance mentioned by Thomson (Chem. Inor. Bodies, vol. II), and formed by decomposing the persulphate of manganese by carbonate of potassa, is most probably an hydrate of one of the oxides.

Phosphoric acid.—When digested upon peroxide, this acid forms a pink solution, giving the per reaction with tests, and which upon evaporation leaves an uncrystallizable pasty mass, of a pink or violet colour, which becomes colourless in a short time, most probably by decomposition.

Boracic acid.—The borate can be readily formed by dissolving the peroxide in boracic acid. The solution thus formed by evaporation leaves a whitish crystalline mass, soluble both in nitric and muriatic acid.

Arsenious acid.—When peroxide is digested in arsenious acid, they unite and form a soluble pinkish white salt.

If bi-arsenite of potassa be digested upon peroxide of manganese, it forms a double salt, being arsenite of manganese and potassa.

Chromic acid.—Chromic acid seems to have no action upon the peroxide, but a chromate may be formed by digesting the peroxalate in chromic acid. The solution is of a dark chesnut brown, but it cannot be evaporated or crystallized, as it is decomposed by the application of heat.

Bichromate of potassa has no action upon the deutoxide of manganese.

Oxalic acid.—This acid has a violent effect upon the peroxide. Oxygen is given off, the insoluble protoxalate is precipitated, while a soluble peroxalate remains in solution. By careful evaporation it may be crystallized, but it is very apt to be decomposed, forming an insoluble salt, most probably the protoxalate. It dissolves in muriatic and nitric acid. It was analyzed by dissolving and precipitating the oxalic acid by chloride of calcium: while another portion was

* Berzelius, *Traité de Chimie*, Tom. IV, p, 170.

calcined and converted into manganoso-manganic oxide. Treated in this manner it showed 27.4348 per cent. of oxalic acid, and 8.5 of manganoso-manganic oxide = 11.73 of peroxide. This leaves a very large per centage for water of crystallization. Thus :—

Oxalic acid,	27.4348
Peroxide of manganese,	11.7300
Water and loss,	60.8352

100.0000

The 11.73 of oxide, requires very nearly 16. of oxalic acid ; which leaves 11.4348 of free acid, so that this salt, in common with the others, possesses a great excess of acid.

If binoxalate of potassa be digested upon the deutoxide of manganese, in the cold, a pink coloured solution is formed, which by standing becomes yellow, letting fall a pink powder. If the solution of the binoxalate be hot, the action is very violent, and the resulting solution is yellow. By evaporation it leaves a crystalline, almost tasteless mass, partly white and partly green, and which is readily dissolved in water.

Acetic acid.—Glacial acetic acid does not form a per-salt when digested on peroxide of manganese.

Tartaric acid.—If this acid be digested upon peroxide, oxygen is given off and the prototartrate is formed. But if we dissolve deutoxide instead of peroxide, a pertartrate results, which on being evaporated leaves the salt of a light yellow or straw colour.

Bitartrate of potassa, particularly if warm, dissolves the deutoxide of manganese with considerable energy, at the same time evolving oxygen, and forming a tartrate of manganese and potassa, which is a highly crystalline brownish mass, of hardly any flavour, and soluble both in nitric and hydrochloric acids.

Benzoic acid.—When benzoic acid is boiled with peroxide of manganese, there is a benzoate formed, slightly soluble in water. Thus obtained, it is a dirty white substance, of a crystalline appearance.

Succinic acid.—This acid forms a protosuccinate when digested upon peroxide, but with the deutoxide, it, like tartaric acid, forms a per-salt, which is soluble in water, of a whitish colour, crystalline and very acid.

Racemic acid.—This, like the last, forms a per-salt with the deutoxide, and a proto-salt with the peroxide. The resulting solution, by evaporation, leaves the salt somewhat crystalline, whitish brown, and quite acid.

Citric acid.—With citric acid, both the per and deutoxide act as towards the last. The percitrate obtained from the deutoxide is a brown, gummy, seemingly uncrystallizable mass, of a pleasant acid taste, slightly deliquescent, and is soluble, although not very readily, in both nitric and muriatic acids.

Gallic acid.—The pergallate of manganese may be obtained by dissolving the peroxide in gallic acid. The solution thus obtained

is of a deep brown colour, and the salt obtained by evaporation is nearly black. It does not appear to crystallize.

These are all the acids of which I have been able to note the action with the per or deutoxide of manganese. I have followed Berzelius in calling peroxide that one which might perhaps be more correctly termed sesquioxide, as its formula is Mn ; but as it is very similar to the analogous oxide of iron, also termed peroxide, and as it is the highest oxide of manganese which forms combinations with acids, it seems best to apply the term of peroxide to this, and super or binoxide to the black oxide of commerce.

If time should favour me, I propose to pursue the above subject, as it is probable that much remains to be determined concerning the compounds of manganese, before we can say with propriety that we are acquainted with the metal.

Philadelphia, May 8th, 1841.

PREPARATION OF CALOTYPE PAPER.

To the Editor.

SIR,—Experiments with the papers of Messrs. Talbot and Hunt, described at pages 382-6 of your last November number, led me to the formation of a paper which I think will be found more sensitive to luminous influence than the calotype. An exposure during two or three minutes, at the distance of a few inches or a foot from a single dip candle, suffices to produce a very evident darkening. Should you deem the description of this paper worthy of a place in the *Annals*, I shall feel honoured by its insertion.

For the sake of brevity, I may state that it is prepared in the same way as the calotype referred to above, with the following exceptions:—The first wash of the nitrate of silver to contain 1·20grs. of that salt to 1oz. of water. The paper so wetted to be quickly dried. Apply the iodide solution to one side only. When the paper is wanted for use, wash it with a solution of ferrocyanate of potash, 100grs.,—1oz. of water: then dip it into rain water. Next apply a “gallonitrate” of double the strength used by Mr. Talbot, and again dip into water. It may be used dry or wet. The impression is brought out by a second wash of the same gallonitrate, but the paper should not be warmed. It is fixed by the use of, firstly, a wash of hyposulphite of soda; secondly, the before-used iodide of potassium. It is better to soak the drawing in the hyposulphite.

I am, Sir,

Your very obedient servant,

G. J. T.

P.S. Either the red or yellow prussiate of potash may be employed. I use the latter.

MISCELLANEOUS.

Messrs. Spencer and Wilson's Patent for Engraving by Voltaic Electricity.

[WE have received a letter from Mr. Spencer in reply to our remarks at page 880, vol. vii, in which, we are glad, to find that the patentees had not been so negligent in guarding against infringements on their patent rights, as, from the tenor of *that* version of the specification which appears in Newton's "Repertory of Arts, &c.," every reader would be led to suppose.—EDIT.]

Alluding to the specification, Mr. Spencer says, "We state several methods by which the object may be accomplished; but as a type of the whole, we say, that the metallic surface to be corroded is to be connected with one end of a slip of metal or wire, while the other end, is in like manner made to communicate with the *negative element* or plate of a single pair. A piece of copper or other metal is to be placed opposite the surface required, to be operated on; and it is in like manner made to communicate, by means of a wire, with the zinc or *positive element* of the same arrangement. This we have illustrated by a drawing, showing how the process is to be proceeded with. The drawing shows two separate vessels, each containing two plates, and letters of reference are on each, in order that all should be legally correct. We also show how the process may be effected, by making the surface to be corroded, or etched, act as the *positive element* of a single pair, by connecting it with a piece of platinum or other metal, having a less affinity for oxygen than the one to be acted on. Although our specification is not voluminous, we have, in fact, shown every method known, to the public, of performing this process. We have also shown how any number of plates may be etched at one time, and not *lose any of their weight*, and yet not be in metallic contact with *any one* of the poles of the battery."

The papers on atmospheric electricity, published in the present volume, were taken from slips of the "*Liverpool Albion*," and supposed to be sent by Mr. Spencer himself. The following note was omitted:—"The paper was read and discussed, on the evenings of the 22nd and 29th of November of the past year, before the Literary and Philosophical Society of Liverpool. It is the author's intention to publish the whole, in a considerably enlarged form, it being at present very much abridged to suit the time occupied by the evening sitting of a scientific society, as well as the necessarily limited space of the columns of a newspaper. The author's object in the present publication is, that, previously, he may be corrected or justified in his views, by other observers."

On the Employment of a Solution of Bichromate of Potassa in Voltaic Batteries.

THE bichromate of potassa has been used for a long time by Mr. Mason, of High Holborn, London, in connection with charcoal, in

the production of flame by voltaic batteries; and a solution of this salt has been recently employed by Professor Leeson, in lieu of a solution of sulphate of copper in what is termed the *sustaining* voltaic battery. The metals of the battery are copper and zinc; the latter amalgamated, and separated from the former by a porous diaphragm. The bichromate solution receives the copper, and a solution of sulphuric acid receives the zinc.

We have not yet heard of any extensive voltaic arrangement in which the bichromate of potassa has been employed; nor, from its great price, is it likely to enter largely into this kind of service. Mr. Mackrell has made a few comparative experiments with twelve pairs of double coppers of the Wollaston form, and amalgamated zinc, in which the latter was amalgamated, and immersed in sulphuric acid solution enclosed in a brown paper case. The liquids in which the copper was immersed were—1st, solution of sulphate of copper; 2nd, solution of bichromate of potash; 3rd, solution of nitrate of potash; 4th, solution of nitrate of soda. All these were saturated. The results of these experiments is said to give a preference to the nitrate of soda, both in the chemical, magnetic, and calorific classes of phenomena. We do not see this verified by the list of results. The sulphate of copper was most active at first, and the bichromate of potash appears to be second in the list, and the nitrate of soda the least active of the whole: and it was not until the expiration of two hours and twenty minutes that the nitrate of soda had any advantage; and what appears singular enough, in Mr. Mackrell's experiments, the battery used with this salt is stated to have an improved action up till four hours and twenty minutes, at which time it produced three cubic inches of gas per minute, from acidulated water, although it only produced '92 of a cubic inch at the end of the first eight minutes!!! The quantity of gas liberated by the sulphate of copper battery was only four cubic inches per minute for the first quarter of an hour; and the whole quantity in four hours' work, is twice that by the bichromate battery, and about two and a half times that given by the employment of the nitrate of potash or the nitrate of soda. In the magnetic and calorific effects, also, the sulphate of copper has a decided preference, for the first hour, at least; but the table of results is far from showing a regularity of action.

[NOTE.—It may be interesting to many of our readers to be informed, that we have found pencils of coke or cinder far superior to any other article in the production of light by voltaic action; and to prevent patent dabblers from taking advantage of it, we at once recommend it, either separately or in conjunction with other matter, as a source of illumination for both *general* and *particular* purposes. With a series of six pairs of cast-iron and amalgamated zinc, we make a massy piece of iron dance in a coil carrying the current.—
EDIT.]

THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND
CHEMISTRY;
AND
GUARDIAN OF EXPERIMENTAL SCIENCE.

JUNE, 1842.

Abstract of a Memoir read at the Sitting of the French National Institute, on the 26th Pluviose, containing an account of some Experiments concerning the Section which Cylinders of Camphor undergo at the Surface of Water; with Reflections on the Motions which accompany this Section. By J. B. VENTURI.†*

ROMIEU formerly observed that small pieces of camphor have a progressive and rotatory motion upon water, and attributed this effect to electricity. Lichtenberg attributed it to the emanation of an ethereal spirit from the camphor itself. Volta produced the same motion of turning by throwing upon water small bodies soaked in ether, or particles of the acids of benzoin and amber. Brugnatelli found that the bark of aromatic plants moves on the surface of water like camphor. There was always a certain mysterious caprice in these motions, according to which they sometimes could not be produced; and on other occasions the motions were instantly stopped when the water was touched with certain bodies, without its being easy to guess the reason. When particles of camphor were fixed to the extremity of a very delicate electrical fly, they produced no motion. All these circumstances tended to envelope the phenomenon with obscurity. Other philosophers in Italy constantly supported the opinion of Romieu in their publications. Citizen Venturi communicated his notions on these facts two years ago at one of the public sittings of the Literary Society of Modena, his country. He was present at the sitting of the National Institute on the 21st Pluviose, when a memoir of Citizen Prevost on the emanations of odorous bodies was read.‡ He announced the observations he had made;

* This paper has been pointed out to us, as a suitable accompaniment to that by M. Dutochet, in our last number.—EDIT.

† Annales de Chimie, XXI, 262.

‡ Philosophical Journal, I, 153.

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and in consequence of an invitation to that purpose, he communicated a memoir of which the following is an abstract :—

Pieces of camphor were cut into the form of small columns, one inch in length ; a base of lead was fixed to each column ; they were then placed upright in very clean saucers, and pure water poured in, to half the height of the column. Two or three hours afterwards, a horizontal notch was manifest in the column of camphor at the surface of the water ; in the course of twenty-four hours, or thereabouts, by the notch becoming gradually deeper, the column of camphor was cut in two at the middle. The two pieces of the column, nevertheless, that is to say, the lower, which was immersed in the water, and the upper, in the air, suffered scarcely any perceptible diminution.

From this experiment, and others made with different pieces of camphor, kept separately in the air, in the water, and at the surface of the water, the author deduces that the most active virtue for dissolving camphor resides at that part where both the air and the water touch the camphor at the same time. Hence he explains, why, in like circumstances, camphor evaporates more quickly in a moist than in a dry air ; and why the Hollanders use water in their process for subliming this substance.

It might be thought that the camphor was decomposed at the surface of the water ; that the water might seize the acidifying part which renders the camphor concrete ; and that the volatile part is dissipated in the atmosphere. The author rejects this notion. He thinks that water with camphor floating on its surface becomes charged with no more than a very small portion : 1. Because in these circumstances the water acquires the same taste and smell of camphor as it obtains when a small quantity of this substance is kept plunged in the same fluid. This water, by exposure to the air, loses the qualities with which it had been charged, and becomes insipid and without smell. 2. Because when the water is saturated with all it can take up, the dissipation of the camphor continues at its surface as before. 3. Because the aerial emanations of camphor made at the surface of water, do themselves crystallise into camphor.

Camphor at the surface of the water does nothing, therefore, but dissolve ; and when dissolved at the ordinary temperature of the atmosphere, it is not at first in a state of vapour, as has been thought. It is simply a liquid which extends itself over the surface of water itself ; and by this means coming into contact with a great surface of air, it is afterwards absorbed and evaporated. This is proved by the following facts : 1. The solution of camphor at the surface of water is more rapid in proportion to the extent of the surface. In narrow vessels, the section of the column would not be completed in a decade (ten days), even though the water might be extremely pure. 2. When the column of camphor has projecting parts, the liquid may be seen issuing by preference from certain points of the column, covering the surface of the water, and driving small floating bodies

before it, in the same manner as floating bodies go and return in a basin into which the water of a canal enters with rapidity. 3. If a small piece of camphor, already wetted at one end, be brought near the edge of water contained in a broad saucer, and be made to touch the saucer itself, it deposits a visible liquor, which is oily, and by attaching itself to the saucer destroys the adhesion between the vessel and the border of the water; so that the water retires on account of the affinity of aggregation, which not being opposed by the attraction of the saucer, causes the water to terminate in a round edge. If you remove the piece of camphor the water will not return to its place until the oily fluid is evaporated. 4. In the same manner, when the column of camphor is half immersed in the water, the oily liquor which issues forth destroys the adhesion of the water to the column, and produces a small surrounding cavity. The solution stops, or is retarded for a moment, until the fluid, extending itself over the water, becomes evaporated: the water then returns to its place, and touches the same part of the camphor; the solution begins again, and in this manner the process is effected by alternations of contact and apparent repulsion.

The rotation of small pieces of camphor at the surface of the water, is simply the mechanical effect of the re-action which the oily liquor, extending itself upon the water, exercises against the camphor itself. If the retro-active centre of percussion of all the jets do not coincide with the centre of gravity, a combined motion of rotation and progression must follow. Since the departure of the oily solution takes place only at the surface of the water, the rotation cannot be effected but round an axis perpendicular to the horizon; and since in similar bodies of different magnitudes the algebraic ratio of the sides to the mass increases in the inverse duplicate ratio of the sides themselves,* the small particles must have proportionally more jets and must revolve more speedily than the larger.

The author reduces to one general rule all the apparent irregularities observable in the motions of the camphor. When these small parts are briskly moved at the surface of the water, if you touch this fluid with any other body, whether conductor or non-conductor of electricity, is of no consequence, provided it be well cleaned from every oily substance, in this case the motion of the camphor will not be affected. But if the same body be afterwards greased by a small drop of fixed oil, or a greater quantity of volatile oil, and then applied to the water at one extremity of the plate, you will discern a scarcely perceptible film advancing at that instant over the whole surface of the water, which will repel the small morsels of camphor, and, as if by a stroke of magic, deprive them of their motion and vivacity. One ounce of oil, poured on one extremity of a bason of

* So in the original. In truth, the sides of like solids are in the inverse triplicate ratio of their solidities. This oversight of the ingenious author does not impair his deduction with regard to the velocities of rotation. N.

water of twenty feet diameter, very soon stops the motion of camphor revolving at the other end. This rapid diffusion even of a fixed oil over a great surface of water, prevents the camphor from extending itself, and stops the solution and rotation of the small particles. It is for this reason, likewise, that particles of sawdust, soaked in fixed oil, begin to turn the moment they touch the water, but cannot continue their motion because the film which they form at the surface of the water is not dissipated in the atmosphere. From this last observation the author deduces this consequence, that volatility and the odorant property are not qualities necessary to produce these rotations. Volatility is merely necessary for their continuation.

At the end of his memoir the author speaks of certain other motions observed in nature, which, by the mechanism of their cause, are in some respect analogous with the motion of oily bodies at the surface of water. In bodies brought near the fire, the humidity retires always to the extremities most remote from the fire itself; because the vapour which is disengaged from the part most heated repels the rest in the opposite direction. So likewise drops of water thrown on an ignited plate of metal, remain, and are agitated in the form of a sphere; because the vapour which is produced by the contact of the plate agitates them, and does not permit them to touch the metal. By the same principle it is very easy to explain the motions of the tremella described by Adanson and Corti, which had inclined some naturalists to rank this byssus in an intermediate class between plants and animals. The tremella is a mass of very minute fibres, which being suspended in the water must be sensible to the slightest impression. It is at present known that plants when exposed to light, decompose the water in their vessels, of which they seize the hydrogen, and extrude the oxygen in the form of gas, by an operation nearly the reverse of animal respiration. If from one of the fibres of the tremella the gas issues on one side, the fibre itself must bend toward the opposite part. If, as in large trees, the gas issues from the tremella in the greatest quantity from the side of the plant which is opposite the light, the tremella must be repelled toward the source of the light itself, to which it always tends when included in an opaque vessel, into which the light enters through a hole. It is not therefore necessary for the explanation of this fact, either that we should suppose any degree of animality in the plant, or attraction at a distance between the oxygen and the light. In the same manner the gas which is abundantly produced by the light of the day, becomes accumulated between the fibres of the tremella, and floats it to the surface of the water. The author takes occasion to express his acknowledgment to Citizen Fourcroy, who afforded him the means of repeating the experiments in his laboratory, and who, by his excellent lessons, informed him of the facts newly discovered since the communication between his country and France had been unfortunately interrupted.

*Additions to the preceding Memoir, in a Letter from the Author to
Citizen Fourcroy.*

CITIZEN :

As I understand that an extract of my memoir upon the section of camphor at the surface of water, is intended to be printed, I request you will add the following facts and reflections :—

1. Dry camphor is very perceptibly volatilised under the weight of the atmosphere, at the 50th degree of Reaumer (145 Fahrenheit). It melts at the 120th degree (302 Fahrenheit), and its volatilization is then extremely rapid. In the Torricellian vacuum it rises even at the ordinary temperature of the atmosphere. This vapour has very little elasticity. It crystallizes along the sides of the tube.

2. A column of camphor is cut asunder much more speedily at the surface of boiling than of cold water. Camphor upon boiling water sublimes in great abundance with the vapour of the water itself.

3. Camphor, when floating upon water, turns and is dissipated by the contact of the oxygen, carbon, hydrogen, and azotic gases. These two last afforded more rapid movements and a more speedy dissipation. They likewise more readily dissolve carbon, phosphorus, and sulphur.

4. When camphor is burned or heated on a float at the surface of water, if it touch the liquid it gives a considerable motion to its support, but if it do not touch the water it remains motionless. The motion is not therefore produced by the simple emission of volatilized particles from the camphor. An action likewise takes place on the part of the water.

5. This action appears to me to depend on the principle of the motion of bodies which float at the surface of water, and has been explained with remarkable perspicuity by Monge. Of two small pieces of paper, twisted up and moistened, the one with the pure water, and the other with water well saturated with camphor, the first attracts and the second repels camphor on the surface of water which does not actually hold that substance in solution. Water consequently has more attraction for solid camphor, than for the little portion which it has already dissolved to saturation. It mounts along the solid piece, where it forms a curvilinear inclined surface. The small portion which is dissolved and saturated, descends along this inclined surface, and in its descent it repels backwards, by the laws of mechanics, both the surface itself and the solid particle to which it adheres. This separation of the dissolved part accelerates the dissipation of the solid piece, by affording a current of water constantly renewed. The atmosphere absorbs that part of the camphor which is already dissolved and extended on the surface of the water ; the evaporation being perhaps assisted by a small portion of the water itself.

6. If a drop of oil had no affinity with the surface of the water

it would lodge itself in a small cavity ; and though more elevated than the surface itself, it would preserve the globular form by its affinity of aggregation. But since it extends in a film over the water, the drop itself, or some of its principles, must necessarily be attracted in the same manner as fluids which rise along the sides of vessels.

7. Air, strongly impregnated with ether, or very hot exhalations of camphor, exercises on the small floating particles at the surface of the water a repulsion similar to that of oil, or of camphor dissolved without heat in the water itself. The former are elastic fluids, and the latter simple liquids. They must not be confounded together.

I have perhaps dwelt too long upon a subject which seems rather to be matter of curiosity, than immediate utility. But it is always a pleasure to me to embrace an opportunity of expressing my esteem and attachment to you.

J. B. VENTURI.

ON VOLTAIC PILES.

(Continued from page 380.)

WE have stated at page 377, that the discovery of the development of electricity by the simple contact of metals, was made by the Rev. Abraham Bennet, prior to the year 1789. This discovery, however, is subsequent to that of M. Sultzer, by applying two metals to the tongue (see page 363) ; but prior to Galvani's discovery of the convulsive motions of prepared frogs, by the application of metallic conductors, as stated also in page 363.

Mr. Bennet's experiments were made by means of an instrument called a *doubler*, which is a modification of the ordinary condenser of Volta. By the employment of this *doubler*, Mr. Bennet was enabled to ascertain that the simple contact of metals produces electric action. The results of this very ingenious electrician's experiments may be pretty fairly represented by the following table.

The following bodies being brought into contact with each other, and afterwards separated, produced positive and negative electric action, respectively, as below :—

Positive.				Negative.	
Lead,	after contact with			Lead ore.	
Lead,	"	"		Iron wire.	
Tin foil,	"	"		Iron wire.	
Zinc,	"	"		Iron wire.	
Iron wire,	"	"		Lead ore.	

Mr. Bennet calls the electric action which bodies are naturally possessed of, with respect to that of other bodies, their *adhesive electricity* ; and states that those bodies which are marked *positive* in the above table, have a *negative* adhesive electricity. His words are

these : " By the experiments of single contact, it now appears that the adhesive affinity of electricity to lead ore is *positive*, and to zinc *negative*." He adds, also, that " gold, silver, copper, brass, regulus of antimony, bismuth, tutenag, mercury, various kinds of wood, and stone, were tried by this method of single contact, and appeared to cause a *positive* charge. Tin was *negative*, and a large piece of zinc much more weakly negative than a thin plate of the same metal used in the preceding experiments."

After Mr. Bennet's experiments became known, they were repeated by Cavallo and others, even prior to the experiments of Volta on the electricity produced by the simple contact of metals ; but those of the Italian philosopher were made in a very different manner, and gave perfectly unequivocal results. Volta, however, as well as his predecessors in these discoveries, employed his condenser in its simplest form ; whilst Bennet and Cavallo had used it in its most complex form of the doubler, whose indications are confessedly not at all times to be depended on. The first set of experiments on record which showed the electric action of metals by simple contact, *without* the aid of a condenser, were published in *Nicholson's Journal* for December, 1804. The following is a copy of the author's own account of these experiments, and of the motives which induced him to proceed with them.

On the Electricity exhibited by Metals, without the help of any Condensing Instrument. By Mr. WM. WILSON.

When I set about making the compound electrical condenser described in my last letter to you, I intended to repeat the experiments of Cavallo, relating to the electricity obtained by the contact of metals related in the third volume of the fourth edition of his *Treatise of Electricity* ; but before the instrument was finished, I was induced (by some experiments I had made relative to the cause of excitation of electricity), to suppose that it is not the contact of the metals that is the cause of the appearance of electrical signs, but the separation of the metals from contact. And this supposition was very much strengthened, when, upon examination, I found that all who have made experiments on this subject have separated the metals from contact before they examined them as to their electricity.

If the contact of the two metals be the cause of the electrical signs, the whole effect that one metal can have on another will be communicated at the time of contact, however few the points are that form the contact, because both of the metals being conductors of electricity, if one possessed a greater proportion of it than the other, a part will be communicated to the other at the time of contact, to form an equilibrium, and this will be done as well by a few points of contact as by a great many. But if it is the separating them from contact, that is the cause of the electrical sign, the more extensive the contact is the more powerful will the signs be when the metals are separated.

To put this to the test of experiment, I pierced a piece of thin sheet copper full of small holes, just big enough to permit to pass through them two or three particles of filings of another metal at a time, so that almost every particle must be in contact with the copper before it can pass through, and consequently the surface of contact be very great with a comparatively small quantity of metal.

I sifted through this copper sieve some filings of zinc into a tin plate laid on the cap of a gold leaf electrometer, and the gold leaves diverged near an inch with positive electricity, when about half an ounce of filings had been sifted into it. Encouraged by this striking result, I procured sieves and filings of different metals. The results of the trials with them are contained in the following table; where P stands for positive electricity, N for negative, and when it was not strong enough to affect the electrometer, that is denoted by O.

Filings of	Sifted through	Effect.
Copper	Copper	O.
	Tin	N. strong
	Silver	O.
	Zinc	P. strong
	Lead	N.
Zinc	Copper	P. strong
	Tin	O.
	Silver	P. strong
	Zinc	P. exceeding weak
	Lead	O.
Tin	Copper	P. rather strong
	Tin	N. very weak
	Silver	P. strong
	Zinc	O.
	Lead	N. weak
Silver	Copper	O.
	Tin	N. weak
	Silver	O.
	Zinc	N.
Steel	Copper	N. very strong
	Tin	N. ditto
	Silver	N. ditto
	Zinc	N. ditto
	Lead	N. ditto

Filings of	Sifted through	Effect.
Lead	Copper Tin Silver Zinc Lead	P. very strong O. P. very strong P. not quite so strong as with silver O.
Brass	Copper Tin Silver Zinc Lead	P. very strong N. strong P. strong O. N.
Bismuth	Copper Tin Silver Zinc Lead	P. exceeding strong O. P. exceeding strong P. strong N.
Antimony	Copper Tin Silver Zinc Lead	O. N. very strong P. strong N. strong N. very strong
Nikel	Copper Tin Silver Zinc Lead	P. exceeding strong O. P. strong O. P.

In all the above experiments there was a large surface of contact, and the electrometer only was used; but in those made by Cavallo and others who had a very small surface of contact, electrical signs could not be made to appear without the help of doublers, multipliers, &c., of electricity. I therefore think there can be no doubt about the separating the metals from contact being the cause of their appearing.

The experiments of Mr. Wilson have been considered of a different character to those of Volta, and are supposed to show the electric action of metals by *friction* against one another, and not by *simple contact* only. And we believe that Dr. Hare, of Philadelphia, has the credit of having first shown the electric action of the voltaic discs, independently of a condenser. Dr. Hare was the inventor of

the single gold leaf electrometer, a far more delicate instrument than the double gold leaf electrometer of Bennet ; and by this instrument the American philosopher was enabled to show the electric action of a copper and zinc pair of discs, by one single contact. The following is Dr. Hare's description of his electrometer, with experiments, as it appeared in the *Philosophical Magazine* for April, 1824 :—

The electrometer consists of a globular glass vessel, which stands on a foot, and having a wide neck at top and a narrower one on one side. The upper neck is furnished with a brass socket, to which is fastened a horizontal disc of zinc, six inches diameter, having its centre directly over the centre of the globe. From the zinc disc hangs a narrow slip of gold leaf, which reaches a little lower than the centre of the globe. Through the *side* neck of the glass passes a brass wire with a ball at each extremity, the inner one of which can be made to approach the pendant gold leaf to any required distance, at pleasure, by means of a micrometer screw. "The electricity produced by the contact of copper and zinc is rendered sensible in the following manner. Place a disc of copper (of the same dimensions as the zinc, and furnished with a glass handle) on the disc of zinc ; take the micrometer screw in one hand, touch the copper disc with the other, and then lift this disc from the zinc. As soon as the separation is effected, the gold leaf will strike the ball, usually, if the one be not more than $\frac{1}{16}$ of an inch apart from the other. I have seen it strike at nearly double that distance. Ten contacts of the same discs, of copper and zinc, will be found necessary to produce a sensible divergency in the leaves of the condensing electrometer.* That the phenomenon arises from the dissimilarity of the metals, is easily shown by repeating the experiment with a disc of zinc in lieu of a disc of copper. The separation of the homogeneous discs will not be found to produce any contact between the leaf and ball. I believe no mode has been heretofore contrived by which the electrical excitement resulting from the contact of heterogeneous metals may be detected by an electroscope without the aid of a condenser. It is probable, that the sensibility of this instrument is dependent on that property of electricity which causes any surcharge of it, which may be created in a conducting surface, to seek an exit at the most projecting termination or point connected with the surface. This disposition is no doubt rendered greater by the proximity of the ball, which increases the capacity of the gold leaf to receive the surcharge, in the same manner as the uninsulated disc of a condenser influences the electrical capacity of the insulated disc in its neighbourhood. It must not be expected, that the phenomena above described can be produced in weather unfavourable to electricity. Under favourable circumstances, I have produced it by means of a smaller electrometer, of which the discs are only $2\frac{1}{2}$

* The gold leaves, if narrow, short, and well insulated, will diverge by three contacts ; sometimes by single contact, when the condenser is used.—EDIT.

inches in diameter. I think I have seen an effect from a disc only an inch in diameter, or from a zinc disc having a copper socket to its handle."—ROBERT HARE.

About the time of the appearance of the above paper by Professor Hare, I was much engaged in researches of this kind, and had occasion to try various plans for showing the electric action produced by the simple contact of bodies; and shortly after I employed a single strip of gold leaf, suspended in the axis of a phial from a zinc disc of about two inches diameter. A hole was made on each of the two opposite sides of the bottle, and through those holes passed two brass wires with a ball at each end. The gold strip thus hung between the two inner balls, which could be adjusted to any distance by screws, as in Dr. Hare's instrument. To the outer ends of the horizontal wires were attached the poles of a dry electric column,* placed horizontally on two glass pillars which stood in a mahogany base board. By means of this instrument I could ascertain the electric condition of the zinc disc after the contact of another metal by the leaning of the gold leaf towards that ball which was in the opposite electric condition. In many cases the gold leaf will strike the ball, when the metals employed are not so large as sixpence. The zinc is moveable, and can be replaced by any other metal. I never insulate the uppermost disc, it having a metallic wire for its handle.—W. S.

Having thus shown the origin and progress of electrical developments by the simple contact of metallic bodies, we will return to those assemblages of metallic bodies constituting the essential elements in the dry electric columns.

In *Nicholson's Journal* for 1810, we find some excellent papers on voltaic arrangements by J. A. de Luc, Esq., with some animadversions on the Council of the Royal Society for not publishing them. With these latter, however, we have nothing to do at present; and shall present our readers with those parts only which explain the theoretical views and experimental discoveries of this excellent philosopher.

Analysis of the Galvanic Pile. By J. A. DE LUC, Esq.

In January, 1806, I had the honour of presenting to the Royal Society two works, connected with each other by the common object of *meteorology*, published at Paris in 1803 and 1804, under the titles of *Introduction à la Physique terrestre par les Fluides expansibles*, and *Traité Élémentaire sur le Fluide Electro-galvanique*. In the latter of these works I had proved, by direct experiments, that

* I was not aware, at the time this instrument was made, that an electric pile had previously been applied to an electrometer. But I afterwards found that Bohnenbergen had applied *two electric columns* to a *double gold leaf* electrometer, about the year 1820. This, however, was fortunately a very different instrument to mine.—W. S.

it was the *electric fluid* itself, which acted in the *galvanic pile*; but that it underwent there a *modification*, which made it produce, with an excessive small quantity, some effects, which else it could not produce but with a very great quantity set in motion by the discharge of the Leyden vial, and even of batteries. This fundamental proposition was established in my work by a particular mode of analysis of the effects of the *pile*, which however other avocations obliged me to publish unfinished, but expressing the hope, that some other experimental philosopher would take it up and follow it; which has not been the case.

In 1807, I saw in *Part I* of the *Philosophical Transactions*, a *Bakerian* lecture of Mr. Humphrey Davy, on *some chemical agencies of electricity*, which revived my attention to this subject. The very ingenious and interesting experiments which distinguish that paper are well known, and my praises would add nothing to those, which it has deservedly received: but a theory was there introduced, which I considered as involving the *electric phenomena* in the thickest veil, and this was my motive for resuming the above experiments. Mr. Davy supposes a *positive* and a *negative* energy, as belonging to distinct *substances*, constituting a class of *general causes*; and in p. 39, after having specified some of the *bodies* to which he attributes these different *agencies*, he concludes thus: "*In the present state of our knowledge it would be useless to attempt to speculate on the remote cause of the electrical energy, or the reason why different bodies, after being brought into contact, should be found differently electrified; its relation to chemical effects is however sufficiently evident: may it not be identical with it, and an essential property of matter?*"

Tutored in Bacon's school, I have found in the long course of my study of natural phenomena, the profound wisdom of the following passage, in his immortal work *De Augmentis Scientiarum*, lib. iii, cap. v. Speaking there "*de occultis et specificis proprietatibus*," which he considers as belonging to a sort of *magic*, the offspring of false *metaphysics*, he says: "*Primum enim intellectum humanum in soporem conjicit, canendo proprietates specificas et virtutes occultas, et tanquam cœlitus demissas, et per traditionum susurros solummodo perdiscendas: unde homines ad veras causas eruendas non amplius excitantur et evigilant, sed in hujusmodi otiosis et credulis opinionibus acquiescunt; deinde vero innumera commenta, et qualia quis optaret maxime, instar somniorum, insinuant.*"*

There is not in the whole field of natural philosophy an object, to which the application of this warning of a true philosopher can

* "First it throws the human understanding into sleep, lulling it with sounds of *specific qualities* and *occult virtues*, as if they came down from Heaven, and could not be learned by the whispers of tradition: whence men are no more solicitous in the discovery of *real causes*; they acquiesce in these idle opinions, and hence numberless gratuitous comments, which, like those on dreams, every man may apply as he wishes."

be of greater importance, than that under consideration. Such *formulæ* as Mr. Davy employs in lieu of *causes*, may appear satisfactory within the narrow limits of the experiments which he describes; but all the spontaneous phenomena of our globe, in which the *electric fluid* is concerned, are thus thrown into the back ground, and never noticed. There is evidently, however, no greater agent in these phenomena, than the *electric fluid*, which Mr. Davy never considers as a *substance*; he speaks only of *electricity*, of *electric energies*, which are empty words in themselves, when supposed to imply the idea of *causes*; while all the meteorological phenomena proclaim a *fluid*, the *chemical affinities* of which already manifested, open the road to the most important enquiries.

This consideration was my principal motive in resuming the analysis abovementioned: I have said, that I had not persued it so far as I saw it possible, but far enough to oppose the erroneous conclusions into which Mr. Davy's obscure principles had led him, with respect to the *mode of action* of the *galvanic pile*; and continuing that analysis by an uninterrupted series of experiments, I arrived at the following propositions, as conclusions of the whole.

1. *Positive* and *negative* in electricity are mere *relations* to a certain *standard*, concerning the distributions of the *electric fluid* among bodies, with which *relations*, no *chemical* effects are connected.

2. The immediate effect of the combination of two proper metals in the pile, and of their repetition, is to accumulate a certain quantity of *electric fluid* on one half of the length of the pile, which the other half loses.

3. When the two extremities of the pile are connected together by conducting substances, the above property of the pile produces a *circulation* of the *electric fluid*, passing constantly from the side on which it tends to accumulate, to the other side, where it compensates the deficiency tending to recur.

4. This *circulation* of the *fluid* produced by a proper association of metals can exist in the same quantity, with the same number of pairs of metals, without either *chemical* effects in the *circuit*, or the *shock*.

5. For the production of these phenomena, it is necessary that a *liquid*, being introduced between the two *metals*, *calcination* be produced on them. In this case the *electric fluid* circulating through the *metals* is *modified*, but its *modification* is different according to the *liquid*: with *pure water*, there are *chemical* effects in the *circuit*, but no *shock* is felt; the latter requires that the circulation be produced by an *acid*.

Such were the conclusions deduced from the experiments related in the paper, which I delivered to the Royal Society, the 30th of May, 1808; but as this and the following paper have not been admitted into the *Philosophical Transactions*, I shall resume here the same course of experiments and deductions, but by more direct steps; being enabled to do so by the progress of my researches from

that time. For this purpose I shall begin by explaining the reason of the plan which I have followed in this enquiry from the beginning in 1800.

If we consider a mounted pile without taking notice of its extremities, we see a repeated association of three constituent parts, two *metals*, and *wet cloth* or *paper*; which I shall name *silver*, *zinc*, and *wet cloth*, as they were in my fundamental experiments. By considering this composition of the *pile*, the leading circumstances for me were, first, that its *effects* increase with the *number* of the repetitions of three constituent parts; secondly, that the whole together forms a *conductive* column. These circumstances concur to point out some cause which accumulates at the extremities of the pile the opposite effects produced in a certain *ternary association* of the component parts: but which is that *association*? In reflecting on this question I was convinced, that, till it was decided, the *mode of action* of the *pile* would remain entirely unknown.

The *pile* may be considered as divided into *ternary groups* under three different aspects:—1, *zinc* and *silver*, and *wet cloth* placed between them; 2, *zinc* and *silver* in *mutual* contact with the *wet cloth* on the side of *zinc*; 3, *zinc* and *silver*, still in *mutual* contact, but the *wet cloth* on the side of *silver*. On this, two questions arose in my mind—first, to which of these *ternary groups* are owing the *accumulation* of the *electric fluid* on one side of the *pile*, and *increase* of its *deficiency* on the other, which become greater with the *number* of these groups? Second, what is the cause, that so small a quantity of *electric fluid*, set in motion by the pile, produces effects which require so great a quantity of the same *fluid* when put in motion by any other means hitherto known?

On the first question, supposing the *conducting* faculty of the *pile* to be the cause of the accumulation of the opposite effects produced on the *electric fluid*, I concluded, that small metallic *conductors* placed between the really *efficient groups*, would not disturb the effects; but that, if these *conductors* were so placed as to produce separate *ternary* associations different from that to which the effects are attached, the latter would cease. Consequently, that by effecting the three different *divisions* of the *pile* by small *conductors*, I could not fail to discover the *efficient groups*—a knowledge which might lead to resolve the second question.

When I proceeded in this plan, I soon found, that no deep analysis of the operations of the *pile* could have been obtained without these admirable instruments, the *gold leaf* electroscope of Mr. Bennet, and the *condenser* of Sig. Volta; so minute are the quantities of *electric fluid* necessary to be observed in the course of these experiments.

After describing an extensive series of experiments in the analysis of the wet pile, M. de Luc arrives at this conclusion:—

The last of these facts leads to the following conclusion. When the *electric fluid* pervades a pile wherein the *calcination* of some *metal* is going on, the *fluid* itself undergoes some *modification*,

which is the cause of the concomitance of these phenomena, a *retardation* of its course through the *water*, and the production, with a very small quantity of *effects* which it cannot produce but with an incomparably greater quantity, when set in motion by any other known means. But before I come to this object, it is necessary to ascertain an essential point, which so far I have only mentioned : namely, that the *motion* of the *electric fluid* in the pile does not depend on a separation of the *binary groups of metals* by a liquid, or a *wet body*; but requires only that the separation be produced by the best *non-metallic conducting substance*; and with the leading experiments concerning this object I shall conclude this *first part of the analysis*.

Exp. 19.—I mounted the pile of 76 *groups* of *zinc* and *silver*; but instead of *wet cloth* to separate them, I used *new cloth*, which had stood some time in my room, my *hygrometer* being at about 40°.

1. *Electric signs* were produced at the extremities of the pile, but weaker than when the pieces of *cloth* were *wet*.

2. The *glass tubes* being applied, these *signs* ceased, but there was no appearance of *chemical effects* in the *water* of the tubes.

Judging that the diminution in the quantity of *electric fluid* in motion proceeded from a want of *conducting faculty* in the *wool* itself, and aware that no *metallic* substance could be substituted for obtaining a better *conductor*, I undertook a long series of experiments, by forming piles of twenty groups *zinc* and *silver*, separated by all the substances of the *vegetable* and *animal* kinds that I could devise, applying these groups to the condenser. Of these experiments, however, I shall only mention the practical result, which was, that among all these bodies, *writing paper* was one of the best for the intended purpose, at the same time that it is the most easy to manage; and I made the following experiment.

Exp. 20.—I mounted again the pile of seventy-six groups *zinc* and *silver*, and separated them with pieces of *writing paper*.

1. I found a great increase in the *electric signs* at the extremities of the pile, comparatively with the *cloth*.

2. These *signs* ceased when the glass tubes were applied, but still no *chemical effect* was produced in the *water* of the tubes.

This experiment opened before me a new and extensive field, in which I have ever since travelled, as will be seen in the *second part* of this analysis.

PART II.

I concluded the first part of this *analysis* by an experiment, in which a pile of seventy-six groups of *zinc* and *silver*, the number used in my preceding experiments, separated by pieces of *writing paper* substituted for the *wet cloth*, though giving very sensible *electric* signs at its extremities, produced no *chemical effects* in the water of the usual glass tubes applied to it, nor the *shock*.

This experiment was made during my investigation respecting the causes of the two distinct effects of the *pile*, namely, *electric* and

chemical, which by the foregoing experiments had been proved to be different; the former depending only on the *binary groups* of the *metals*, separated by the best *conducting* substance *non-metallic*, and thus not necessarily productive of *chemical* effects; these requiring absolutely, that the interposed *non-metallic* substance be *wet*, a circumstance from which the *chemical* effects depend on *groups* composed of the *two metals* with the *wet body* between them. Such is the fundamental proposition, with respect to the *mode of action* of the *galvanic pile*, which was to be submitted to farther experiments.

The *conducting* faculty of all the substances of the *vegetable* and *animal* kinds, that may be employed to separate the *binal groups* of *metals*, for transmitting from one to another their individual *electric* effects, mostly depends on a certain quantity of *moisture*, with which they are usually penetrated. Of this I had soon an opportunity of being informed, by an experiment of my friend Dr. Lind, who had constantly followed the progress of these researches; he had constructed a pile similar to mine, except that *copper* plates were substituted for *silver* plates, which acted also immediately upon the gold leaf electroscope; but he found the following change in it.

Exp. 21.—He dismantled this pile, and laid all its parts on his hearth, before a great fire, so that the pieces of paper were almost singed, and mounting it again in this state, it did not sensibly affect his electroscope: he brought it into my room, and it affected only the electroscope of my condenser. We then dismantled the pile, and laid all its parts on a table for one hour, my hygrometer being about 40°, and when again mounted, it acted on the electroscope as it did before the *papers* had been so thoroughly *dried* by a great heat.

This experiment shows that Mr. Davy was mistaken when he thought "that with respect to *electricities* of such *low* intensities, *water* is an *insulating* body." On which principle he considered the *mode of action* of the *pile* under the idea of what he calls *induction*; meaning, I suppose, something similar to the effect of Volta's condenser, when it operates by the interposition of *silk* between the *metallic* plates. If this were the case, pieces of *silk*, through which what he calls *induction* is readily produced, being placed between the binary groups of *metals*, should produce a still greater effect than *paper* in its common state, as more *insulating*; whereas the foregoing experiment proves, that the effect almost ceases, when *paper*, by thorough *dryness*, is reduced to the state of an *insulating* body. I insist on this point only, because it involves one of the most important questions in terrestrial physics. Must we, in the *present state of our knowledge*, be satisfied with *electrical energies*, which might be considered as *essential properties of matter*?* Or rather, in this very *state of our knowledge*, is it not already ascertained, that a particular *substance* exists, namely, the *electric fluid*,

* Mr. Davy's *Bakerian Lecture*, *Philosophical Transactions*, 1807, part I, p. 39; or *Journal*, vol. XIX, p. 50.

which, beside the effects here in view, produces greater and more general effects on our globe? This question is the main object of this and the following papers, and I hope it will be decided by facts.

Having found no *conducting* substance more convenient than paper, for transmitting from one to another of the binary groups of *metals* their individual effects, I came to consider whether it might not be of some advantage, on account of the very small quantity of *electric fluid* thus set in motion, to produce a closer contact of the *paper* with the *metals* between their groups. I made various experiments, by pasting the *paper*, first on both *metals*, then on one only, in the intervals where I had placed it loose. My condenser rendered easy these trials, as I could make them on piles of only twenty groups, and the following was the general result. There is an *increase* in the *transmission* of the *electric fluid* when the *paper* is pasted on the outside of such group upon the *metal* which becomes *negative*; but the reverse takes place, when it is pasted on the *metal* which becomes *positive*.

In the following experiments each pile of twenty groups was placed upon the moving metallic pillar of my condenser, and raised into contact with the receiving plate of the latter, where I left it during twenty seconds; then, letting it down, and removing the upper plate, I observed the divergence of the gold leaves, and noted it in *decimals* of an inch, as in the experiments of the 1st *part*. In these operations, the end of the small piles, which rested on the pillar communicating with the ground, was thereby *neutral*; and the whole *electric difference* between the extremities was expressed, with its proper *sign*, by the electroscope of the condenser. The first experiment on each association of *metals* was made with the papers *loose* between the *groups*, in order to observe afterward the differences resulting from the *pasting* them on one or the other of the *metals*.

Exp. 22.—With twenty groups *zinc* and *silver*, separated by *loose* pieces of *paper*.

Silver side on the *pillar*, *zinc* side applied to the *cond.*

Zinc side..... *silver* side *pos.* 0·35.
..... *neg.* 0·35.

The *difference* between the *extremities* being commonly equal, with only opposite *signs*, when these *extremities* are in turn applied to the condenser, I shall suppress the *sign* when the *quantity* only of the *difference* will be the object of consideration.

Exp. 23.—This will give the first proof of the advantage of pasting the *paper* on the *metal* which becomes *negative* in contact with the other; this being the case of *silver* in the above pile.

20 groups *zinc* and *silver* separated by *loose* paper 0·35
20 groups *paper* *pasted* on the *silver*..... the *gold*
leaf struck the side.

Zinc and *silver* (or *copper*) are, as yet, the *metals* which, being associated, differ the most in their *electric* states, and for this reason

they are commonly used in the pile; but in order to ascertain whether the advantage manifested by the above experiment, of pasting the *paper* on the *metal* which becomes *negative* in each group was general, or only a circumstance attached to *silver* and *zinc*, I thought of employing some *metal*, which became *negative* with *zinc*, but *positive* with *silver*; I found *pewter* such an intermediate metal, from the following experiment.

Exp. 24.—With two *piles*, each of twenty groups, the first *zinc* and *pewter*, the second *pewter* and *silver*; the groups separated by loose pieces of *paper*; each *pile* being alternately placed in communication with the *ground* by one side, while the other communicated with the *condenser*.

1st pile	{ <i>zinc</i> side applied to the condenserpos.	0·10
	{ <i>pewter</i> sideneg.	0·10
2nd pile	{ <i>pewter</i> side applied to the condenser...pos.	0·20
	{ <i>silver</i> side neg.	0·25

This experiment first affords a new proof, that *negative* and *positive* are no *qualities* attached to certain *metals*, since here *pewter* is *negative* in its connexion with *zinc*, and *positive* when connected with *silver*. It is only an accidental circumstance, that there is as yet no known substance with which *zinc* becomes *negative*, and none with which *silver* (or *copper*) becomes *positive*; and they have in themselves no tendency to either of these states, acquiring them, and that inversely, by particular circumstances, as will be seen hereafter.

With respect to my present purpose, this experiment having shown me in *pewter* a metal proper for the trials concerning the pasting of the *papers*, I made the following experiments.

Exp. 25.—With 3 piles, each of 20 groups *zinc* and *pewter*.

1. The *paper* pasted on *zinc*almost no effect.
2. loose between the *groups*..... 0·1
3. pasted on *pewter* 0·5

Exp. 26.—With 3 piles, each of 20 groups *pewter* and *silver*.

1. The *paper* pasted on *pewter* 0·05
2. loose between the *groups* 0·2
3. pasted on *silver* 0·4

Though there is not so much increase of effect in the last as in the two preceding experiments, it shows still the same general circumstance, that there is some advantage for increasing the transmission of the individual effects of each *group of metal* to the extremities of the *pile*, in fixing the *non-metallic conducting substance* on the outside of each *group*, upon the *metal* which becomes *negative* in its connexion with the other; a circumstance which led me soon to farther steps, for these experiments presented me first the prospect of a *spontaneous*, as well as lasting *electric machine*, the

power of which could be increased by a greater number of *groups*.

It would have been too expensive to proceed in that attempt with *silver* plates, and I was going to undertake it with *copper* plates, pasting the *paper* over them, when fortunately I thought of a *paper*, called *Dutch gilt paper*, on which a kind of *copper* is ready laid. At first I did not find the paper of that sort which is plain, but having tried that which is flowered, and finding much effect, I made the following experiment.

Exp. 27.—I cut seventy-six pieces of this *paper* to the size of my *zinc* plates, 1·6 inch diameter, and having formed a pile of seventy-six groups, with only the *zinc* plates separated by the pieces of this paper, the *copper* side of all of them turned the same way, I found more *electric* effects at its extremities, than with the former pile of the same number of groups of *zinc*, *silver*, and *wet cloth*.

This experiment increased my hope of obtaining a *natural electric machine*, not indeed of a great power, but new in experimental philosophy, and which might lead to some farther discovery; but unluckily for speed, being sedentary at Windsor, I was not yet informed that, by a mode rendering *zinc* ductile, thin plates could be obtained of it, which would answer my purpose, since in this use, calcination was not to be apprehended. Wishing however to proceed some way in this attempt, and having found by trial with my condenser, that *tinned iron* plates had sensibly the same effect as *pewter* plates, I procured as many plates of the former, 1·6 inch diameter, as would fill up the two columns of my frame, separated as above by pieces of flowered *Dutch gilt paper*. There were four hundred in each column, thus composing a pile of eight hundred groups.

Exp. 28.—The first object I shall mention of this experiment strongly corroborates the former conclusions concerning the *mode of action* of the *galvanic pile*. Though this pile had at its extremities greater *electric* effects than I have ever seen at those of any pile producing the strongest *chemical* effects in the *glass tubes* with *water*; yet these *tubes* being applied, there was not the least appearance of such effects.

But at the same time a new scene was opened to my view. This natural *electric machine* soon gave me reason to hope that it might become a new and very essential *meteorological* instrument. Having no motive to dismount it, I left it a long time in the same state, observing the *electroscopes*, and the following are the general phenomena which they offered to me.

1. The quantity of *electric fluid* put in motion by this pile was too great for the gold leaves to remain with a simple *divergence*; one of them *struck* the tinfoil on its side, then fell, and *struck* again; which effect became the object of observation.

2. At times these *striking*s were at both extremities of the pile, alternately. When the gold leaf *struck* on one side, it placed this extremity for an instant in communication with the ground; the gold leaf fell, but this communication with the ground increased the

opposite electric state at the other extremity, where one of the gold leaves *struck* and fell ; and this continued by turns.

3. At other times the *striking*s were only on one side, and continued perhaps one day or two ; then that state changed, and the *striking*s were at the other side.

4. At which ever side the *striking*s were going on there was a great difference in their *frequency* : sometimes the gold leaf seemed to beat *seconds*, while at other times it *struck* but once in a minute, or a longer time.

5. When the *frequency* of the *striking*s was upon the whole nearly equal many consecutive days, it increased succesively from the morning till some time in the afternoon, and then went on diminishing till night.

6. Sometimes I connected one side of the pile with the *ground*, by a metallic rod, which made the gold leaves fall on this, and increased the *frequency* of the *striking*s on the opposite side ; but this increase was neither always the same, nor equal in the same time, when the communication with the *ground* was changed from one side to the other.

7. The beginning of my observations of this kind was in the winter of 1808, and in this season the *striking*s were upon the whole very frequent ; but as the spring advanced, and perhaps as vegetation increased on the ground, there was a gradual diminution in the motion of the *electroscopes*, and a change in the effects on the pile from the communication of its extremities with the ground.

8. Lastly. These changes could not be attributed to those of either *heat* or *moisture*, at least in my room, and as influencing directly the pile ; for the room remained nearly at an equal temperature, and there were but small changes in my hygrometer. I also frequently observed the barometer, and found no connexion between its variations and these great changes in the *striking*s of the gold leaves.

From the whole together of these observations, I could attribute the variations in the *electric* phenomena of the pile only to changes in the *electric* state of the *ambient air* ; or those in the effects of the communication with the *ground*, to changeable differences between the *electric state* of the latter and that of the *air* ; an object on which we owe to Sig. Volta a great and fundamental truth. This justly celebrated experimental philosopher has made it evident that *air* possesses the *electric fluid* as well as all the bodies which it surrounds ; and on this principle he has founded the only real explanation of the *electric motions* of free bodies, such as the pairs of balls and the gold leaves of our *electroscopes*. The *electric fluid* tends to an *equilibrium* among all bodies, including the *air*, and there is a mutual attraction between it and all of them, weak with *conductors*, but strong with *non-conductors*, among which is *air*. The proportional quantity of *electric fluid* which is *actually* possessed by *air*, is the *standard* of *plus* and *minus* in the *electroscopes* ; and the proportional quantity of this *fluid*, which is *actually* possessed by

the *ground*, is the *standard* of *plus* and *minus* concerning the differences in the *electric state* of insulated bodies compared with its own: these *standards* are sometimes similar, but they are often different. I shall not enlarge here on this beautiful theory, which I have fully explained in my works, and demonstrated by direct experiments;* but applying it to the pile, the immediate effects of the *ambient air* must be to give some *electric fluid* to its *negative* side, which has proportionally *less* than itself, and to take some from the *positive* side, which has *more*; and as this influence must change according to the changes in the *electric state* of the air, the modifications of the *electroscopes* at the extremities of the pile may lead to unfold the latter, but not without farther discoveries.

I was stopped in the progress of these researches by an incident which has occasioned me much labour, and still retards them. This new *pile* is certainly in itself a *meteorological* instrument of great importance, as may be already judged, and shall be farther explained in the following paper; but in the state above described it was not fit for regular observations, and till the present moment† I have not yet surmounted all the difficulties. I foresaw them, and it was the reason why I wished that the beginning of this new career should be soon known to experimental philosophers through the *Phil. Transactions*, in order that it might be followed by others, and probably with more success than by myself; but I am reduced to give the history of my own progress.

From what has been above explained, the *striking*s of the *gold leaves* in the *electroscopes* were become the object of observation: for this purpose, therefore, they ought to have regularly continued; however, after one of the *gold leaves*, at either side, had alternately *struck* and *fallen* for some time, it at last *stuck* to the tinfoil. The side, therefore, to which this happened, was placed in permanent communication with the *ground*, which made the *striking*s to begin at the opposite side; but there also the gold leaf *stuck*. These adhesions continued, till, by a stroke on the top of the *electroscope*, the the gold leaf fell; the oscillations were then renewed, but again stopped in the same manner. I tried various methods for preventing this adhesion, especially by substituting for the tinfoil, which has a rough surface, polished laminæ of many sorts and forms, keeping them even at some distance from the glass, which might contribute to this effect; but all was to no purpose, still the gold leaf would *stick*. This was a great disappointment, and the only method I could devise was, to increase the power of a pile, till it could move small metallic balls, in hope that these would not *stick*.

Two ways presented themselves to my mind for this increase of power in a pile; one by using *plates* of a larger *size*, the other by

* *Idées sur la Météorologie*, published in London by Elmsley in 1786 and 1787.

† Two years have elapsed since I communicated to the Royal Society the experiments and observations which are here my object.

increasing the *number* of the *groups*. The first method would have been cumbersome ; but for a reason which I shall explain, I did not expect that it would have the desired effect. However, even for the verification of my conjecture, I made the following experiment.

Exp. 29.—I procured 10 *tinned iron* plates, 4 inches square ; 10 others round, of only 0·5 inch diameter ; and I took 10 of my plates of 1·6 inch diameter. I cut pieces of *Dutch gilt paper*, the size of each of them, and compared their respective effects on my condenser. The result was beyond my expectation ; I had only conjectured that the increase of *size* would not increase the *divergence* of the gold leaves ; now this was not only verified, but the largest plates produced the smallest effect. However, this unexpected difference probably proceeded from some accidental cause, which I had no time to investigate, and I considered this experiment, which I have often repeated under various forms, only as ascertaining the following proposition : that for the *electric motions*, considered solely with respect to the quantity of *divergence* in the *electroscope*, the size of the *plates* is indifferent ; though, for the *frequency* of the *striking*s of the little pendula, and the intensity of the effects when the extremities of the pile are connected together, with the same *number* of *groups*, these effects increase with the *size* of the *plates*. This distinction, to which I shall return in the following paper, constitutes a part of the theory, which, as it made me foresee the result of the above experiment, I shall now explain.

I am indebted for the ground of this theory to Sig. Volta, who, when in 1782, he showed me, at Paris, his then new-invented admirable *condenser*, explained to me, that it could not serve to manifest minute degrees of *deviation* from the *electric standard*, when belonging to *small* bodies, but only to bodies of such an *extent* or *nature*, that the application of the *condenser* (by this taking its share of that *deviation*) does not sensibly lessen it. As an example of the first case, he gave me the atmosphere ; and with respect to the *nature* of bodies, he took a *Leyden phial* discharged without a continued contact, the *residuum* of which, from its *nature*, may affect a moderate sized *condenser* without being much lessened : and to show me the necessity of this condition, he made use of the following analogy. When a piece of ground, by being swampy, indicates some stagnant water, if a *well* be dug there, the water will not fill it up to the level of the *stratum* of earth whence it proceeds, unless this *stratum* be of such an *extent*, that the quantity of water which gathers in the *well* has no sensible proportion with that contained in the stratum ; so that the subtraction of this quantity cannot affect the *level* at which the water stands in the *stratum*. We have also an example of this case in the subject of electricity : when an insulated *electrified* body is small, we cannot know its real degree of electrification by applying a common *electroscope* ; because this, sharing the *deviation* of the state of the body from the *electric standard*, lessens it too much for expressing what it was before that application.

I shall use the former of these examples in explaining my theory concerning the difference of effects of the *size* of the *plates*, and the *number* of the *groups*, according to the use of the *pile*; and this explanation will chiefly consist in fixing the points of *analogy* between the two objects.

1. I compare the *number* of *groups* in the *pile* to the *elevation* of a *stratum* whence water issues into a *well*.

2. The *size* of the *plates*, to the *extent* of this *stratum*.

3. The *degree* of *divergence* in the *electroscopes* at the extremities of the *pile* to the *level* which the *water* can attain in the *well* without *overflowing*.

These first *analogies* are sufficient to explain the case of the last experiment. When we attend to what is directly expressed by our *electroscopes*, we certainly do not expect that this instrument shall indicate the *quantity* of *electric fluid* possessed by the bodies to which it is applied; for this would require also to measure their surface; we expect only to know the comparative *density* of the *electric fluid* among bodies, or its power to produce certain degrees of *divergence* in the *electroscope* we use.* Such therefore are the indications of the *electroscopes* at the extremities of the *pile*: they express certain *degrees* of *density* of the *electric fluid* on them, which are the same whatever be the *area* of the *plates*; these *degrees* depending only on the *number* of the *groups*, because each *group* contributes to increase the *density* of the fluid on one of the extremities by lessening it on the other. Thus it is that the *divergence*, both in *plus* and *minus*, of the *electroscopes*, at the extremities of the *pile*, is proportional only to the *number* of *groups*, in the same manner that the *height* at which *water* stands in a *well* is proportional only to the *elevation* of the *stratum* whence it proceeds.

4. But when, in order to produce a *current* of water, a pipe is placed, or a trench is cut, on the side of a well, below the *level* at which the water stood in it, the *current* will be greater and more permanent, in proportion to the *extent* of the *stratum*, of the same *elevation*, whence the water proceeds; and also, when the *divergence*

* I shall take this opportunity of explaining why I use the word *electroscope*, and not that of *electrometer*; it is because there is no instrument entitled to the latter denomination, at least admitted among experimental philosophers. Indeed, of our instruments serving to measure the degrees of intensity of physical causes, I know none *absolute*, except *hygrometers*, such as have been constructed by M. de Saussure and myself; for though these instruments are made of different substances, and differ in some other respects, we have obtained in both an *absolute zero*, and *absolute maxima* in determined cases, as well as determined degrees of intensity, of their object, namely *moisture*: all the other instruments intended for the same purpose, to my knowledge, are only *hygroscopes*, indicating *variations* in *moisture*, without determined *points*, or *degrees* common to them. Thus no kind of *physical instrument* has yet obtained the conditions of an *absolute measure*, but the above *hygrometers*, so little thought of by experimental philosophers, though very important in meteorology. The *thermometer* has obtained two *fixed degrees* of *heat*, and determined *divisions* of the interval between them,

of the little *pendula* of the electroscope exceeds the extent that it can have without one of them *striking* the side, then *falling* by a momentary contact with the tin foil, which communicates with the ground, it will *sooner* rise and *strike* again, with the same *number of groups*, in proportion to the *size of the plates*; which last circumstance increases also the *current of electric fluid* circulating in a *pile*, the extremities of which are connected together by a conductor.

5. The *water* of all *springs* has the same source, namely, the *rain water* percolating through the ground, and retained on some impervious *stratum*, either argillaceous or stony. If this *water* do not find in its way any substance with which it can combine, it comes out as it had fallen on the ground: but if in its course it combines with any substance, it may come out with certain *chemical* properties, different according to the substances which have combined with it. The case is the same with respect to the *electric fluid* which pervades the *pile*: its source is no other than the *electric fluid* diffused over all terrestrial bodies, therefore over the *pile* itself. However we should be ignorant of the constant existence of this *fluid*, over us and around us, were it not that, by artificial or natural operations, its *density* may either be increased or diminished on *insulated* bodies: this is the only circumstance which makes it appear, and that by the *electroscope* alone; for as long as this *fluid* remains in a state of equal diffusion over all bodies, it is manifested by no effect hitherto discovered. The *friction* between two bodies disturbs that *equilibrium*, in a manner which I shall show in a future paper on the *Analysis of the Electric Machine*. But in the *pile*, which is my present object, it is by a property of its composition, that the *equilibrium* of the *electric fluid* is disturbed, whence proceed either the motions of the *electroscopes*, or a *circulation* of the fluid through the *pile*, when the extremities of the latter are connected together by a *conductor*. Now, in the last of these cases, if the *electric fluid*, in its course meets with no substance that changes its state; as is the case in a pile composed of *tinned iron*, or *zinc* plates, separated by *Dutch gilt paper*; we are indeed informed by the *electroscopes* of its *accumulation* on one extremity of the pile, and

by which means experimental philosophers understand one another when they indicate certain *degrees of heat*. I have also constructed an *electrometer*, which possesses the same conditions with respect to *degrees of electrification*, which is described in my work, *Idées sur la Météorologie*; but not having been attended to by experimental philosophers, I have not been induced to follow the extension of this *measure* down to the minute degrees of intensity indicated by the gold leaf *electroscope*, as I could not expect that it should be more noticed: therefore admirable as is this instrument for its sensibility, it affords us no *comparable measure*. In this imperfect state, however, there is, in every *electroscope*, a property which belongs to no other *physical measure*, namely, a *natural and absolute* standard of *plus* and *minus*, which is *constant*, as to its general determination, and is the *actual* electric state of the *ambient air*, or the *ground*; though *variable* as to the absolute quantity, as are these electric states; which difference will be one of the objects of this paper.

its deficiency on the other ; however, neither *chemical* effects in the *circuit*, nor the *shock*, are produced ; because the *fluid* remains unaltered : but when it pervades a *pile*, wherein, by a *liquid* being placed between the two *metals*, there is *calcination* of one or both of the latter, new effects appear : if the *liquid* be *pure water*, *chemical effects* are produced in the *circuit*, but there is no *shock* ; if it be an *acid*, both effects are produced.

These experiments, especially on the different effects of the *number* of the *groups*, and of the *size* of the *plates*, with the above theory on the cause of their different effects, were contained in my paper delivered to the Royal Society the 30th of May, 1808, about one month before Mr. J. G. Children executed in presence of Mr. Davy and Mr. Allen the grand experiment of the same kind related in *Part I* of *Ph. Trans.* for 1809, by which the theory which I had already announced was confirmed.

But here two questions arise, which go deeper into the *mode of action* of the *galvanic pile*, and they are these : 1. Of what *nature* is the *modification* produced in the *electric fluid*, when it pervades a *pile* wherein the *calcination* of some *metal* is going on ? 2. What is the *cause* of the *motion* of this *fluid* in the *pile*, whether producing, or not producing the *shock* and *chemical* effects in the *circuit* ?

The solution of the former of these questions, which leads to that of the latter, depends on the *nature* of the *electric fluid* ; a subject much too long to be treated here ; but it is fully detailed in both the works I have already referred to ;* I shall therefore here confine myself to the conclusions contained in these works, as deduced from uninterrupted series of experiments, of which I shall only detail the part necessary to my subject.

None of the phenomena observed in our common electrical experiments, namely, the *charge* and *discharge* of the *Leyden vial*, the *electric motions*, the effects of the *electrophorus* and of the *condenser*, had been really explained, till the inventor of the last two instruments, Sig. Volta, had formed his theory on the *electric influences*, which threw the first true light on the modifications of the *electric fluid* ; and which, in the course of various experiments I made to follow it through all the electric phenomena, gave rise to the system on the *nature* of the *electric fluid*, which I shall here briefly state.

This *fluid*, far from being a *simple* substance, is an astonishing *compound* : and first, in its state which may be called *natural*, that, I mean, in which it is diffused over all bodies, it is found composed of two main parts, from which all the above-mentioned phenomena arise. One of these two constituent ingredients of the *electric fluid* in this state is a substance, which, by itself, is not *expansible* (as in *steam*, also an *expansible fluid*, there is a substance which is not *expansible* by itself, namely *water*) ; this substance in the *electric*

* *Idées sur la Météorologie, and Traité élémentaire sur le Fluide électro-galvanique.*

fluid I have called *electric matter*; and its function, which I shall soon point out, is very distinct. The other ingredient is an excessively subtle *fluid*, which (as *fire* in *steam*) uniting with the *non-expansible* substance, produces the *expansibility* of the aggregate. In my French works I have called the latter *fluide déferent*; but here I shall call it *vector*, a short word of the same import, signifying that it *carries along the electric matter* (as, in *steam*, *fire* is the *vector* of *water*).

The electric *vector* instantly pervades all bodies, and carries the *electric matter* through *conductors*, but not through *non-conductors*, such as *glass* and *resinous substances*: when a current of *electric fluid* arrives on one side of a *lamina* of these substances, and its *vector*, in order to establish its own *equilibrium* beyond it, pervades the *lamina*, it deposits the *electric matter* on the surface of the latter, where it remains adherent, till a current of *vector* pervades the *lamina* in the opposite direction, or it is taken up slowly by the *vector* in the air (as *fire* in *steam*, when it pervades a glass *lamina* to establish its own *equilibrium* beyond it, deposits the *water* on the side which receives the *steam*, where it remains, till it is carried away, either by *fire* coming from without, or by that spread in the air).

I come to the peculiar function of the *electric matter* in the above indicated phenomena: it is the sole cause of *electric motions*, resulting from a *greater* or *less* proportional quantity of it, than is possessed by the *ambient air*; to which subject I shall return: the *vector* has no share in these *motions*, but as the *vehicle* of the *electric matter* acting in their phenomena. (As, with regard to *steam*, it is only *water* that produces the *hygroscopic* phenomena, without any interference of *fire*, except as the vehicle of *water*).

By this system of a first *composition* of the *electric fluid*, the phenomena, which I have introduced in the beginning, are clearly explained in all their modifications, as I have abundantly proved by direct experiments in my works. But as long as the *electric fluid* remains in what I have called its *natural* state, moving along *conductors* and fixed on *non-conductors*, it produces no *chemical* effect hitherto known: what then does happen, when it produces these phenomena?

If we attend to this change, we shall observe a circumstance *sine qua non*, which is to contain some *cause*; it is, that the *conductor*, along which the *electric fluid* moves, *must be interrupted*. Now, when in this case the *electric fluid* darts through the *air*, three new phenomena are observed, *lucidity*, *heat*, and a particular *odour*. This cannot but indicate the *decomposition* of some particles of the fluid, occasioned by an excess of density, from which *light*, *fire*, and an *odorate* substance are disengaged: as when *steam* (to which from the beginning I have compared by analogy this system on the nature of the *electric fluid*) becomes too *dense* for the actual *temperature*, some of its particles, being *decomposed*, emit *water* and *fire*.

These new substances, *light*, *fire*, and an *odorate* substance, thus manifested in the composition of the *electric fluid*, are neither the

electric matter, nor the *vector*, themselves, but must be contained in them, combined with some other substances, which prevent them from exercising their characteristic effects; a case most common in chemical compounds. The characteristic effect of *fire*, is *heat*; when *free* it acts upon the *thermometer*; but it does not, when combined with other substances. *Lucidity* is the characteristic effect of *light*; but this is not *lucid* in phosphori, till they are *decomposing*: and also various bodies, while *decomposing* emit *odorate* substances, which in their compound state had no *odour*. Now, the *light* emitted by the *electric fluid* probably belongs to the *vector*, which has many properties of the former; but it is not *lucid*, therefore *light* must be combined in it with some other ingredient. The *odorate* substance appears to belong to the *electric matter*, but this has no *odour*, therefore the former must also be combined in it with some other substance. Lastly, the *fire* emitted cannot be referred directly to either the *vector* or the *electric matter*; but probably, during their common *decomposition*, it is itself composed of the *light* and *igneous matter* disengaged. That *fire* is a *compound*, is a system which I have also treated with many experimental details in the above mentioned works.

No natural philosopher, who has applied to the study of any main branch of terrestrial phenomena according to the rules of analysis instituted by the immortal Bacon, will be repulsed by the idea of so many *elements* entering into the composition of the *electric fluid*, though hitherto almost excluded from the catalogue of *chemical substances* by a class of chemists who confine their observations within their laboratories. When, with the view of ascending from some of the most common phenomena to general causes, we have followed this scrupulous analysis by a certain number of regular steps, we are yet, in almost every branch, stopped for want of intelligible *links*, though in series of phenomena manifestly connected together by some common cause; as for instance many phenomena manifested in our chemical operations, with some which we daily observe in the atmosphere, that great laboratory of nature on our globe. The filling up of these chasms by gratuitous hypotheses is only protracting the attainment of real knowledge.

Let not therefore natural philosophers lose sight of an *expansible fluid*, constantly associated with all terrestrial bodies, and with the *air* that surrounds them; this present in all our chemical processes, during which some of its *ingredients*, either *engaged* or *disengaged*, might account really for certain phenomena hitherto explained by mere words. For, according to meteorological observations which I shall relate in the following paper, it is by its *decompositions*, alternating with *compositions*, that the *electric fluid* operates in terrestrial phenomena. What were chemical theories before the *chemical combinations* of *fire* with other substances were discovered and attended to? However, as long as this *fluid* shall be considered under the vague idea expressed by the modern word *caloric*, it will not much forward the science of chemistry.

After these general remarks, I return to my subject, which will serve as an example of their application. When the transmission of the *electric fluid* through *interrupted* conductors takes place in a *liquid*, the new phenomena of *lucidity*, *heat*, and *odour*, are not perceived; but there cannot be any doubt, that the *chemical* effects produced in the *circuit*, and the *shock*, proceed from the same *decomposition* of particles, that takes place at *interrupted* conductors, which is visible only through the *air*; for no *chemical* effect is produced in the *water* of the glass tubes, when the *metallic wire* passes through it *uninterrupted*. With respect to the *shock*, this condition is not immediately perceived in the discharge of the *Leyden vial*, because it is sudden, attended with a strong commotion, and not repeated till the *vial* is again *charged*; but with the *pile*, which soon renews spontaneously the cause of the *shock*, it has been seen in Exp. 8, that this phenomenon is produced only at the *approaching* contact, and thus by an *interruption*; since all sensation ceased when I *fixed* the silver spoons on both extremities.

These preliminary deductions of facts were necessary for the solution of the first of the questions above stated, namely: "Of what *nature* is the modification produced in the *electric fluid*, when it pervades a *pile* wherein the *calcination* of some *metal* is going on?" A question intimately connected with this: "How does it happen, that, with such a minute quantity of *electric fluid* set in motion by the *pile*, the *shock* and *chemical* effects are produced, while they require a very great quantity of the same *fluid* when set in motion by any of the other known apparatuses?" Being arrived at the general fact above stated, that these effects are never produced but by the *decomposition* of some particles of the *electric fluid*, occasioned by an excess of density, in darting from one point of a *conductor* to another, the answer to the connected questions is obvious: the *modification* undergone by the *electric fluid* in pervading this *pile* is such, that some of its particles are *decomposed* by a very small increase of *density*, when a *conductor* is *interrupted*. We have an analogy of the general case of more easy *decomposition* of *compounds* by previous modifications of the latter, in the processes of *smelting ores*, for obtaining *metals* or *reguli* from them; for an easy separation of the ingredient of the latter must be prepared by *subtraction* or *addition* of other ingredients, and often by both. And as we see that the *calcination* by an *acid* is necessary to produce the *shock*, it is probable, that the modification of the *electric fluid* in this case is the addition of some *element*.

I come now to the second of the above questions: "What is the *cause* of a *motion* of the *electric fluid* in the *pile*, either producing, or not producing, the *shock* and *chemical* effects in the *circuit*?" The first point to be considered with respect to this question concerns the *nature* of the modifications reciprocally produced by *zinc* and *copper* upon each other, when brought into contact. It is generally said, that, in this case, *ZINC* becomes *positive*, and *COPPER* *negative*. But these expressions, according to what has been stated

above, cannot relate to the *expansive power* of the *electric fluid*; for, between two bodies in mutual contact, such a *power* must be in *equilibrium*; therefore these expressions must relate to *density*, such as I have defined it. Let us now attend to the experiments.

These trials are usually made with discs of *zinc* and *copper* (or brass) having an insulating handle in their centre, like the plate of an *electrophorus*, and with the help of the *condenser*; thus, one of the discs being held on the hand, the other is first laid upon it, then brought, by its insulating handle, into contact with the *condenser*. It is commonly supposed that the latter must be *touched* while it lies on the other; but this, assimilating the process to that of the *electrophorus*, changes the nature of the phenomenon, and may lead into error. I shall therefore first relate the experiments which I have made, with discs of the two metals about four inches diameter, from which the difference between these effects will be seen. But I must premise, that in these experiments, the results are very various at different times with respect to the *quantity* of effect; I shall therefore relate first such experiments as I set down in a certain part of one day; then mention the differences.

Exp. 30. I held the *copper* plate on my hand, and laid it upon the *zinc* plate, which I *touched* with my finger in this situation, and then carried to the *condenser*: after twenty repetitions of these alternate contacts, the divergence, then *positive*, of the gold leaves of the latter, was about half an inch.

Exp. 31. I made the same experiment without *touching* the *zinc* plate: the gold leaves struck the sides. I repeated the same process with only ten alternate contacts, and the same quantity of effect was produced as with twenty, when *touching* the *zinc* plate.

I must now mention that at other times I have found no difference of effect from either *touching* or not *touching* the *zinc* disc while lying on the *copper* disc; and that at different times the quantity of effect in both operations was smaller; a point to which I shall return: but these first experiments prove directly, that when *zinc* is in communication with *copper*, the former takes from the latter some *electric fluid*, making other bodies, on its opposite side, share its *excess*. Thus, after a certain number of repetitions of the alternate contacts, when the upper plate of the condenser is removed, the *fluid* accumulated on the *receiving* plate is manifested by the electroscope: and instead of having increased the effect by *touching* the *zinc* on the *copper*, the finger has sometimes, even during that contact, taken off a part of the accumulated *fluid*. I come to the reverse experiment, made also at a certain part of one day.

Exp. 32. I took the *zinc* disc on my hand, and, placing the *copper* disc on it, I *touched* the latter before carrying it to the *condenser*, which it made *negative*. I repeated the experiment without *touching* the *copper* disc lying on the *zinc* disc, and the *negative* effect was of the same quantity. In general, a greater number of alternate contacts is required to produce the same quantity of *negative* effect with *copper*, than of *positive* with *zinc*.

We see however in this experiment the same phenomenon as in the preceding, in this respect, that when *zinc* and *copper* are in mutual contact, the former takes some *electric fluid* from the latter, which, on its opposite side, it shares with other bodies : in the first experiment it shared this *fluid* with the *condenser*, while the *ground* restored it to the *copper* disc ; in the last, *zinc* communicating that acquisition to the *ground*, took more *fluid* from the *copper* disc, which made the *condenser* partake of its *loss*. This is the leading thread with respect to the *motion* of the *electric fluid* in the *pile*, and I shall follow it ; but I must first speak more particularly of the anomalies observed in these experiments, which are important.

These differences, sometimes very great, in the quantity of effect of the same operations, surprised me at first ; and suspecting something amiss in the *condenser*, I examined it closely, without finding any defect : then at other times, without any change, I found the same effects. At last I remarked that, commonly, the greatest effects in the same day were in a part of the morning, and the smallest towards the evening ; and that these effects differed also in intensity on different days. Now this is what I have said above of the spontaneous effects of the *pile*, which is composed of a succession of the same *binary* groups of *metals* ; and thus the *condenser*, when applied to these experiments, is also a *meteorological* instrument. And there is a remarkable circumstance in this respect ; that often at the same moment there is a great difference between the effects on the condenser of the opposite extremities of a small pile ; but sometimes it is the *negative* side which prevails, and at other times it is the *positive*. This manifests, that the *ground*, with which the opposite side of the small pile communicates, in the former case possesses *less*, and in the latter *more* of the *electric fluid* than the *ambient air*. But this will be one of the subjects of the following paper.

After having shown distinctly what are the *electric* effects produced by the conjunction of *zinc* and *copper*, and in what manner the condenser manifests these effects, I come to their propagation from each group along the *pile*, on one side *negative* and on the other *positive*. The following experiments will show the first steps, which will be easily extended through the whole.

Exp. 33.—Upon a *zinc* disc, of the same size as the others, I fixed a piece of *Dutch gilt paper*, the *copper* side next to the *zinc*, and the *paper* outside, held by a little paste all around. This association is to represent *one group* of the *pile*, considered for the present only on the *copper* side with the *paper*, which, in the pile, separates it from the next *group* on this side, and more directly from the *zinc* plate of that *group*. Holding on my hand the *zinc* side of the former *group*, I repeated on its *paper* with the insulated *zinc* disc, the alternate contacts with the condenser ; and by twenty repetitions a sensible *negative* divergence was produced in the electroscope of the latter. The same effect takes place in the *pile* at the *copper* side of every *group* ; it takes, through the *paper*, some *electric fluid* from the *zinc* of the *next group* ; but as *zinc* must always possess more of this *fluid* than the *copper* with which it is con-

nected, the *zinc* of that *next group* takes more from its own associated *copper*, which then through the *paper*, takes more from the *zinc* of the following *group*; which effect goes on increasing, up to the end of the *pile* on this side. I shall only add to this experiment, that, as the property of a group composed of *zinc*, *copper*, and *paper* on the latter, is to have this side *negative*, whatever be the *metal* with which the above alternate contacts are performed, they produce the same *negative* effect on the *condenser*.

In order to complete directly the first step here in view, by taking one *group* as an example, it would be desirable, that by placing a piece of paper on the *zinc* side of this *group*, in order to represent the separations of the *groups* in the *pile* in that direction, it should manifest directly by the *condenser*, that *zinc* yields some *electric fluid* to the next *group*; but I have not been able to obtain such a direct proof, on account of a circumstance which I shall now indicate, as peculiar to that quantity of *electric fluid* set in *motion* by the property of the *pile*. This *motion* is very *slow* compared with the *motion* of an external quantity of *electric fluid*. The smallest degree of electrification of an insulated body, *positive* or *negative*, such as can only affect the gold leaf electroscope, communicated to one side of a *pile*, is instantly manifested at the other extremity; but it is by no means the same with respect to the *electric fluid* set in motion by the property of the *pile*: when the electroscopes diverge equally at the extremities, having touched one of them, which makes the gold leaves fall here, and rise more on the opposite side, it requires a time, often very long, before the same divergence is restored. This is the reason why I was obliged to fix twenty seconds for the duration of the contacts of my piles of twenty groups with the *condenser*, in order to be certain that the *maximum* of effect was produced; it requires more or less *time*, according either to their nature, or to the extremity applied to the *condenser*; but I found, that the slowest was produced in twenty seconds, which made me fix that time.

This will explain the case abovementioned, that there is no sensible effect on the *condenser* by any number of alternate contacts of an insulated body, with the paper laid on the *zinc* side of one group; and even when the *groups* are multiplied to increase the effect, it requires, to make it sensible, a prolongation of the contact on the little pile, in order to give *time* for the effect to be propagated; as will be seen by the following experiment.

Exp. 34. Made with a portion of my pile of ten groups of 1.6 inch diameter, successively tried by each of their extremities, the opposite one being placed on the movable pillar of my *condenser*, and the alternate contacts being made with a small insulated disc of the same diameter.

1. No number of alternate contacts, between either of the extremities of the little pile and the *condenser*, produced any sensible effect on the latter when rapidly made, or in the manner they are executed between the two naked discs, *zinc* and *copper*.

2. On repeating the experiment, with the *zinc* side on the *pillar*,

and lengthening the *time* of the contact on the *copper* side up to four *seconds*, I found, that by twenty such contacts the electroscope was affected *negatively* to a certain measurable quantity.

3. Inverting the little pile, I had no sensible effect by these contacts of four *seconds* on the *zinc* side, and it was necessary to lengthen the time to eight *seconds*; but in order to produce on the electroscope a *positive* effect equal to the *negative* of the preceding trial, I was obliged to make forty contacts, on account of the dissipation of the effects on the condenser in each interval of time. This experiment shows however, sufficiently, that the *zinc* side of each *group* yields, through the *paper*, to the next *group*, some of the *electric fluid* that it takes from the copper with which it is associated.

We have from these experiments all the elementary principles necessary for the *motion* of the *electric fluid* in the pile, and they are the following:—1. In each *binary group*, the *zinc* plate takes some electric fluid from its associate the *copper*; the latter in my new *pile*, being the *coppered side* of the *Dutch gilt paper*. 2. In each *group* also, *zinc* communicates, through the *paper*, some of its excess of *fluid* to the *copper* of the next *group* on its side. 3. In each *group* again the *copper* takes, through the *paper* from the *zinc* of the next *group* on its side, some of the *fluid* that it has lost to its associated *zinc*. The same effects taking place in every *group*, with the next on both *sides*, along the whole *pile*, these effects are successively added to those that the respective *next groups* have already undergone according to their *place*; and thus the *negative* state goes on increasing from one end to the other of the *pile*, toward what is called the *copper* extremity; and the *positive* state is increasing toward the *zinc* extremity.

These effects may be represented by *numbers*; though, from the great variations in the quantity at different times, and the imperfection of the electroscopes, these *numbers* remain undetermined: I shall express them in a *pile* of eleven *groups*, indicating by A the *zinc* side, and by B the *copper* side. The two following series represent the progress of *negative* and *positive* effects above mentioned, which, combining in each successive *group* from A to B, constitute, according to circumstances, the three different states of the *pile*.

A	A
0	+10
— 1	+ 9
— 2	+ 8
— 3	+ 7
— 4	+ 6
— 5	+ 5
— 6	+ 4
— 7	+ 3
— 8	+ 2
— 9	+ 1
—10	0
B	B

In the *insulated* pile, when the *divergence* of the electroscopes is equal on both sides, *positive* at A, and *negative* at B, the state of each successive group is the *sum* of the corresponding *numbers* of the above series, as expressed in the 1st of the following Tables. When B is placed in communication with the *ground*, the losses of all the *copper* plates being repaired by the latter, all the *acquisitions* of the *zinc* plates subsist without *diminution*, which requires the quantity ten, to be *added* to each number of Table I, as expressed in Table II. When A communicates with the *ground*, all the *acquisitions* of the *zinc* plates being carried into the latter, the *losses* of the *copper* plates remain uncompensated, and the same quantity, ten, is to be *subtracted* from all the numbers of Table I, as expressed in Table III.

TABLE I.
Insulated pile.

A
+ 10
+ 8
+ 6
+ 4
+ 2
0
— 2
— 4
— 6
— 8
— 10
B

TABLE II.
B in com. with the gr.

A
+20
+18
+16
+14
+12
+10
+8
+6
+4
+2
0
B

TABLE III.
A in com. with the gr.

A
0
—2
—4
—6
—8
—10
—12
—14
—16
—18
—20
B

Now this synthesis of the above fundamental experiments is the real fact; as will be seen from direct experiments in the following paper, of which I shall give here only the general results.

For these experiments I use a horizontal *pile*, which I have called *column*; with a gold leaf electroscope at each extremity; and I have also a detached electroscope, which may be applied and observed at every point of the *column*. The following are the observed phenomena:—1. When the state of the ambient air is such, that in the *insulated column* the divergence is equal at both extremities, the *middle* point in its length is *zero* as represented in Table I, in which, as well as in the two others, the *terms* are to be considered only as equidistant *points*, whatever be the number of the *groups*. 2 When B communicates with the *ground*, the first plate only at this extremity is *zero*, and the *positive* state is gradually increasing towards A: the *middle* point is *plus*, of the same quantity (sensibly) as it was at A in Table I, and the divergence *plus* is doubled at A; a state represented in Table II. 3. When A communicates with the *ground*, all the effects are reversed: the first plate only at A is *zero*, and the *negative* state is gradually increasing towards B: the *middle* point

is now *minus*, of the same quantity (sensibly) as it was at B in the case of Table I, and the quantity *minus* is doubled at B; a state represented by Table III. I do not know any theory on *invisible causes*, which more exactly follows the *visible effects*.

What remains to be considered is the *physical cause* of these phenomena, all originating in this circumstance, that when *zinc* and *copper* are in mutual contact, *zinc* possesses more *electric fluid*, and *copper* less, than in another situation. In my first paper delivered to the Royal Society, I explained this effect by analogy with the phenomenon of the different *capacities* of bodies for the *fluid cause of heat*; but having here entered into an explanation of the nature of the *electric fluid*, I shall derive analogies from the subject itself.

Most of the electric phenomena manifested in our experiments depend on the distinction which I have established between the *density* of the *electric fluid*, consisting in the proportional quantity of *electric matter* and its *expansive power*, depending on the quantity of *vector*. This distinction is particularly manifested by the *changes* that happen along an insulated *conductor* of some length, when an electrified body is placed at some distance from one of its extremities. It is known in general, that the extremity of such a *conductor* next to an *electrified body* acquires an *electric state* contrary to that of the *body*, while its opposite extremity has the same electric state as that body: for instance, suppose the electrified body to be *positive*, it is commonly said, that the extremity of the *conductor* next to this body becomes *negative*, and the opposite extremity *positive*; but these are vague expressions, and as they give no real idea of the effects, they have occasioned the variety of systems, all unsatisfactory and therefore changing, hitherto made on these phenomena.

The cause of obscurity on this object is the want of that distinction above mentioned, between the *expansive power* and the *density* of the *electric fluids* belonging to its nature, as *expansible*. Every fluid of this class, when confined within a certain space, has necessarily the same degree of *expansive power* in every part of this space, since this is attached to the very idea of *expansibility*; but it is not the same with respect to *density*. For instance: a mass of *air*, confined in a certain space, has certainly, in all its parts the same *degree of expansive power*, whatever change may happen in its partial *density*. If then a *hot body* be placed near one side of this mass of *air*, its *density* will *diminish* in this part, and *increase* in the more remote: or if a piece of *ice* be brought on one side of this mass of *air*, its *density* will *increase* near the *ice*, and *diminish* in the remote parts; but the *degree of expansive power* of this confined *air* will always be equal in all its parts at the same time, *increasing* or *diminishing* in the whole.

The case is exactly the same with respect to the *electric fluid* on an insulated *conductor*: an insulated *positive* body being placed

near one part of the latter, this part receives some of the *vector* which forms around the *positive body* a kind of atmosphere, as the *hot body* has around it an atmosphere of *igneous fluid*: and the former produces an increase of the *expansive power* of the *electric fluid* on this part of the *conductor*, as the *hot body* produces it in the *air* next to it. A *negative body* produces also an effect analogous to a piece of *ice*: for as this absorbs a part of the *free fire* in the parts of the mass of *air* next to, so the *negative body* absorbs a part of the *vector* from the *electric fluid* on the nearest part of *conductor*. Now, all these changes in the *degree of expansive power* in both *fluids* are attended with inverse changes in their *density*.

Nobody will doubt of the above statement who has repeated the experiments described in my works; and I may say, that every one of the great number of persons, in whose presence I have made these experiments, with the set of small electric instruments described in my *Traité élémentaire sur le Fluide Electro-galvanique*, has been convinced of this important distinction between the *density* and *expansive power* of the *electric fluid*. It is not therefore for want of progress in the science itself, that such a variety of systems subsists concerning the *electric phenomena*; it is for want of attention in most experimental philosophers, by whom, though writing on electricity, my published experiments are never mentioned, not so much as to criticise them, or their conclusions. Among the variety of these experiments, I have here chosen those concerning the modifications which take place on a long insulated conductor; and I come now to the particulars, which will prove all that I have stated above.

Three *electroscopes* are used in that series of experiments, one of which, a *movable* one, consists of silver *laminæ*, suspended on small axes, in order to prevent their motion out of the line of their divergence. Of the two other *electroscopes*, consisting of small pith balls suspended by silver wires, one is permanently connected with the extremity of the *conductor* furthest from the *electrified body*; the other, held up at the top of a high insulating pillar, is kept, by means of a thin wire, in constant communication with the *movable electroscope*: the latter, though principally destined to move along the *conductor*, may be removed, from every point, to a distance, in order to try the *nature* of the *divergence* that it had at this point, which it does not lose on removal. Lastly an insulated brass *disc* is the *electrified body*. The following are the phenomena observed, in which must be recollected what I undertake to prove, namely, that the *divergences* of the *electroscopes* are produced by *plus* or *minus* of *electric matter* only, comparatively with the *standard*, which is the *actual* proportionate quantity of *electric matter* possessed by the *ambient air*; an object connected with the proof of the fundamental proposition, that there is an absolute distinction between the *density*, and the *expansive power* of the *electric fluid*.

1. At the beginning of the experiment, the *movable electroscope*

is placed near that extremity of the *conductor*, to which the disc, after having received a spark from a Leyden vial, is to be approximated. At the approach of this *positive body* within a small distance, the silver *laminæ* of this *electroscope* diverge as *negative*; some of their *electric matter* having receded to the remote parts of the *system* (by this word I express the *conductor* and its associated *electroscopes*); and thus, though the *expansive power* of the *electric fluid* has equally increased upon its whole extent, the two remote *electroscopes*, and in particular that which is connected with the silver *laminæ*, diverge *positively* by a certain quantity.

2. When the silver *laminæ* are made to recede from the *positive body* along the conductor, their *negative* divergence gradually diminishes; it ceases at a certain distance, and farther than this begins a *positive* divergence, which increases to a certain *maximum*; nevertheless the remote *electroscopes*, and in particular that which is in immediate connection with the *laminæ*, remain *positive* to the same degree as at first. The effect, therefore, of withdrawing the *laminæ* has been to remove them out of the atmosphere of *vector* of the *positive body*, which, by increasing the *expansive power* of their *electric fluid*, had made a part of their *electric matter* to abandon them: and it is the *electric matter* retired from the anterior part of the *system* which occasions the *positive* divergence in the remote *electroscopes*.

3. While the *movable electroscope* is thus removed from the atmosphere of the *positive body*, if any part of the *system* be touched with a small wire held in the hand, the *expansive power* of its *electric fluid* is thus placed in *equilibrium* with that of the *ground*, and the *divergence* ceases in all the *electroscopes*. Which shows, that, in the parts of the *system* over which the atmosphere of the *positive body* does not extend, the *equilibrium* of proportional quantity of *electric matter* has been also produced. If then the silver *laminæ* be moved toward the *positive body*, when they arrive within that atmosphere they begin to diverge as *negative*; and this divergence continuing to increase, it is greater when this *electroscope* arrives at the end of the *conductor*, than it had been in the same place at the beginning of the experiment. However, the remote *electroscope* in communication with this remains without divergence; because the small quantity of *electric matter* newly withdrawn from the silver *laminæ* is insensible upon the whole *system*.

4. Now will come a proof (among many others which may be found in my works), that the *divergence* in the *electroscopes* depends *only* on the proportional quantity of *electric matter*, or *density* of the *fluid*. If, in this state of the *system*, the silver *laminæ* be touched with a wire held in the hand, though this contact places them in communication with the *ground*, their *divergence* continues the same: because their *electric fluid*, by the increase of *vector* proceeding from the *positive body*, being in *equilibrium* of *expansive power* with that of the *ground*, no *electric matter* can ascend to them

from the latter, and they remain *deprived* of it to the same degree.

5. Lastly. If the *positive body* be removed or discharged, the three electroscopes diverge as *negative*. By the contact of the *system*, during the influence of the *positive body*, which had increased the *expansive power* of the *electric fluid* over the whole, the quantity of *electric matter* which had retired from its anterior parts had passed into the *ground*; and now, when the influence of the *positive body* has ceased, this *deficiency* of *electric matter* becomes common to the whole system.

Having now, by this series of experiments, demonstrated the fundamental proposition, that an *equilibrium* of *expansive power* of the *electric fluid* may subsist, between two insulated bodies in mutual contact, with a difference in its *density*, or proportional *quantity* of *electric matter*, I come to my system of the *electric states* of *zinc* and *copper*, when, being insulated, they are in mutual contact, which system is founded on that proposition.

It is evident, that the *electric fluid* must be in *equilibrium* of *expansive power* on this group composed of *zinc* and *copper*; therefore, the *difference* observed in their *electric states* must proceed from a difference in the *density* of the *fluid*. Now the only hypothesis added in my system to this immediate conclusion from fact is this: that, during their connexion *copper* has the property of acquiring more *vector* than *zinc*, from that diffused in the *ambient air*; by which proportional increase of *expansive power*, the *electric fluid* on *copper* is in equilibrium with that on *zinc*, though with less *density*, or proportional *quantity* of *electric matter*. I have shown also in the above mentioned works, by deduction from experiment, that, when this *influence* of bodies on each other, by *plus* or *minus* of *vector*, has ceased by sufficient *distance*, each of them possess instantly, by the effect of the *ambient medium*, a quantity of *vector*, proportional to its quantity of *electric matter*; and thus it is, that the modifications produced by *zinc* and *copper* on each other while associated, and their effects on bodies brought into contact with them on the outside of their *groups* during their association, are converted into modifications of the *quantity* of the *electric fluid* itself.

After having treated here the theoretical part of the subject more fully than I had done (for brevity's sake) in my first paper given to the Royal Society, in order to be better understood on this subject, very important in natural philosophy, I return to the experiments concerning the *analysis* of the *galvanic pile*, to bring them here to the same point as they were at in that paper.

Having found, by the experiment related at the end of the first part of this analysis, that by increasing the *size* of the *plates*, the *divergence* did not increase in the electroscopes, I considered the manner in which a great number of small plates might be used. I thought then of having a hole in the centre of small plates, in order to thread them with silk in form of *chaplets*, alternating the plates

with equal pieces of *Dutch gilt paper*. Not yet having any but *tinned iron plates* for these trials, I formed two such chaplets, each composed of 140 *groups*, of 0.5 inch diameter, and in order to guard them against dust, I enclosed them in glass tubes: but I found, that, when the chaplets lay along the glass, the effect was diminished, and in order to prevent this defect, I took larger tubes, with metallic caps, through which, by screws, I kept the chaplet fixed in the axis of the tube; these screws, being on the outside in the form of hooks, served to link the chaplets together. Each of these small instruments acted immediately on the gold leaf electro-scope, and, by hooking them together, the effect was doubled.

I founded at that time, on this experiment, the plan of increasing the power of the new instrument so as to produce the divergence of small metallic balls, especially by using *zinc plates*, which I knew then could be procured. For this purpose I thought of lessening the expense occasioned by the metallic caps for the glass tubes, by making much longer *chaplets*, and of suspending these in the form of garlands, to the ceiling of the room, by silk strings, bringing only wires from their opposite extremities to a proper place, where the apparatus of the small metallic balls should stand.

Such is the point at which I had arrived the 30th of May, 1808; and this object was much forwarded in my paper under the title of *The Electric Column and Aerial Electroscope*, delivered to the Royal Society the 7th of March, 1809; but the Committee of Papers not having ordered it to be published in the *Phil. Trans.*, it will now appear in a more advanced state. The consequence of its not appearing at the time it should have done is, that my *electric column* has lost the merit of novelty; for, by the communications of the minutes at the meetings of the society and the committee, it had attracted attention; and the sight of it, which I did not refuse, has made it sufficiently known to be already imitated. However, while its connexion with the analysis of the galvanic pile is not considered, its principle object is lost: and moreover, till the *sticking* of the small *metallic pendula*, when *striking*, was prevented, which I have obtained but lately, it could not be ranked among *meteorological* instruments. These particulars will be seen in my following paper.

(To be continued.)

Researches on the action of the Alkaline Peroxides on the Metallic Oxides. Letter from M. Ed. FREMY to M. PELOUZE.*

THE evident analogy which exists between iron and manganese would lead to the supposition that we may one day produce with iron all the compounds which have been obtained with manganese.

It is for this reason that I have thought that it would be possible to form salts in which an oxygenated compound of iron would play

* *Compte Rendu.*

an electro-negative part, and which would thus correspond to combinations of manganic and permanganic acid with the bases.

The facts I am about to make known show, I believe, in a conclusive manner, that iron can give birth to bodies which are produced in the same manner as the manganates, and which present a striking analogy with this latter class of salts.

When we heat for a considerable time to a temperature of a bright red, a mixture of potash and peroxide of iron, we obtain a brown mass which, dissolved by water, gives a solution of a very beautiful red violet, and which possesses characters hereafter to be indicated. This compound may be prepared more easily and in a few minutes, by calcining at a very high temperature a mixture of nitre, of potash, and peroxide of iron; or still better a mixture of peroxide of potassium and peroxide of iron.

I have succeeded in reproducing this body by the humid method, by causing a current of chlorine to pass into very concentrated potash, holding in suspension the hydrate of the peroxide of iron.

This compound obtained by these different processes is of a beautiful violet colour; a great quantity of water decomposes it at length; it becomes insoluble in very alkaline water, forming then a brown precipitate, which dissolves easily in pure water, and gives a solution of a beautiful purple colour. It appears much less stable than the manganate of potash. Under certain circumstances it is decomposed at the ordinary temperature in peroxide of iron, which becomes precipitated in pure oxygen, which disengages itself; and in potash, which becomes free: the liquor is in this case completely discoloured. A temperature of 100° makes it undergo a similar decomposition, but which is instantaneous.

All organic matters decompose it: it is consequently impossible to filtre its solution.

Such are the different facts which I have proved, and which all tend to make us admit the existence of a body more oxygenated than the peroxide of iron. I have found it impossible hitherto to isolate this compound, for when we come to treat this red solution by an acid, when the potash is saturated, there is a disengagement of oxygen and a precipitation of the peroxide of iron. If the acid is in excess it dissolves the peroxide, and forms a salt of the peroxide of iron.

In the memoir which I shall have the honour to present to the Academy, I purpose giving the analysis of these new compounds, which I found based on the complete decomposition which they undergo under the influence of a temperature of 100° , and which permits us to proportion the disengaged oxygen, and the peroxide of iron which is precipitated.

I shall discuss the different manners of considering their composition in examining whether we may admit in these bodies the existence of alkaline peroxides. I shall also make known the results which I have obtained by placing the other metallic oxides in the same circumstances.

As the bodies which form the object of my researches are of a delicate preparation, and the study of them ought to be of a prolonged nature, I have thought it my duty to take date, and to announce from to-day the first results which I have obtained.

On the Constant Voltaic Battery. By J. F. DANIELL, For. Sec. R.S., Prof. Chem. in King's College, London; in a Letter addressed to R. Phillips, Esq., F.R.S., &c.*

MY DEAR SIR,—IN the *Annales de Chimie et de Physique* for December, 1841, there is a paper by M. Edmond Becquerel, entitled “*Sur les piles à Courant Constant*,” upon which I beg your permission to make a few observations in the *Philosophical Magazine*. The object of the author is thus stated :—

“A l'époque actuelle, où l'on cherche de toutes parts à appliquer les sciences physiques et chimiques, et par conséquent l'action des forces électriques, aux arts industriels, je pense qu'il peut être utile de présenter succinctement l'exposé de toutes les recherches qui ont été faites pour obtenir des piles dont l'action soit constante pendant un certain temps.” (*Ann. de Chimie*, tome iii, p. 436).†

In this opinion I perfectly concur; and it is for the purpose of rendering such an account more perfect that I desire to correct some misconceptions into which M. E. Becquerel has fallen, and which have the effect of greatly mystifying the origin of constant batteries. After stating the well-known defects of voltaic batteries of the common construction, in which the two metals are plunged into the liquid in the same cell; and the necessity of constructing batteries of constant action before they could be usefully employed in the arts, he proceeds to make the following comprehensive claim :—

“C'est mon père qui a donné le premier les principes sur lesquels est fondée la construction de ces piles et qu'a formé les premiers piles de ce genre.” (*Ann. de Chim.* p. 437).‡

Now as I have claimed to be the inventor of “the constant battery” (which was so named by myself), and as the Council of the Royal Society have so far sanctioned this claim as to award me the Copley Medal for my invention; and as I have most undoubtedly worked out the principles of its construction by experiments and legitimate induction, without the slightest suspicion that M. Becquerel had preceded me in the investigation, I was naturally very anxious to examine the evidence upon which this assertion is founded.

* From the *Philosophical Magazine*.

† “At the actual epoch, in which it is sought in all parts to apply the chemical and physical sciences, and consequently the action of electrical forces, to the industrial arts, I think that it may perhaps be useful to present succinctly the exposition of all the researches which have been made to obtain piles whose action should be constant during a certain time.”

‡ “It was my father who gave the first principles on which the construction of these piles is founded, and who formed the first piles of this kind.”

Previously to stating this evidence, M. E. Becquerel makes some remarks upon the phenomenon which has been most inappropriately termed the *polarization* of the plates of a voltaic battery, of which his father, he observes, has given the simple explanation.

“ Dans le passage d'un courant électrique à travers un liquide conducteur voici ce qui a lieu : quand le courant primitif traverse le liquide conducteur, des élémens acides sont transportés au pôle positif, et des élémens alcalins au pôle négatif ; alors, en interrompant la communication entre les lames décomposantes (ou électrodes) et la pile, les deux lames se comportent vis-à-vis l'une de l'autre comme deux lames que l'on aurait plongées, l'une dans un faible dissolution alcaline, l'autre dans une faible dissolution acide : c'est-à-dire, qu'en les mettant en relation avec un multiplicateur, il y aura production d'un courant électrique dû à la réaction des molécules acides ou se comportant comme telles, sur les molécules alcalines par l'intermédiaire du liquide : ce courant sera par conséquent dirigé en sens inverse du courant initial. Cet effet se manifeste toujours lorsqu'un courant traverse un liquide conducteur quelconque au moyen de deux lames métalliques. Cette polarisation des électrodes a donc lieu dans toutes les piles, lorsque les lames qui compose chaque couple plongent dans un même liquide et dans un même auge.” (Pp. 437-8).*

This hypothesis, however, though it borders upon the true explanation, is not correct ; for in my first paper upon “ Voltaic Combinations ” (*Phil. Trans.*, 1836, p. 116), I have shown that the polarization of the plates, and the rapid decline, and final cessation, of the current in batteries of the common construction, is owing to the deposition of metallic zinc upon the conducting plates. This I at first ascribed to the deoxidating power of the hydrogen upon the oxide of zinc, but have since adduced evidence to prove it is owing to the direct electrolysis of the sulphate of zinc formed. The deposit I was able to detach from the platinum, upon which it had been precipitated, in plates of considerable thickness. This opposition of

* “ In the passage of an electric current across a liquid conductor observe what takes place ; when the primary current traverses the liquid conductor, the acid elements are transported to the positive pole, and the alkaline elements to the negative pole ; then, on interrupting the communication between the decomposing plates (or electrodes) and the pile, the two plates act in opposition to each other, like two plates which are immersed, the one in a feeble alkaline solution, and the other in a feeble acid solution ; that is to say, by putting them in connection with a multiplier, we shall have the production of an electric current due to the reaction of the acid molecules, or which act as such on the alkaline molecules by the intermediation of the water : this current will consequently be directed in an inverse direction to the initial current. This effect always manifests itself when a current traverses any liquid conductor whatever, by means of two metallic plates. This polarisation of the electrodes takes place then in all piles, when the plates which compose each pair are immersed in the same liquid and in the same trough.”

zinc to zinc in the acid is sufficient to account for the result without supposing "the reaction of alkaline matter transported to the conducting plate upon acid transported in like manner to the zinc plate through the intervening fluid." It is difficult, indeed, to conceive that the latter should not rather increase the power of the direct current, by its action upon the zinc with which it must be in contact, than produce a counter current by its action upon the alkaline matter upon the distant platinum plate.

The problem of a constant battery, which M. E. Becquerel says that his father has solved, is thus stated :—

"Si donc on pouvait enlever continuellement les élémens alcalins et acides qui se déposent sur les lames, on anéantirait le courant secondaire, et la pile aurait une intensité constante, en tant cependant que les surfaces des lames resteraient à peu près aussi nettes et que l'action du liquide sur le zinc serait à peu près la même." (P. 438).*

It appears from the statement of M. E. Becquerel, that in the year 1829 his father contrived an apparatus consisting of a glass cell or rectangular vessel, the interior of which was divided into three compartments by two diaphragms of gold-beater's skin, admitting of no communication except through the membranes placed for the purpose of retarding the mixture or combination of the liquids contained in each.

The bottom of the cell was perforated only in the centre compartment by a small opening, so that upon plunging the cell into another vessel containing a conducting fluid, the liquids in the two extreme compartments would mix with great difficulty : the central aperture admitted also of being closed, and the middle compartment could then be filled with the liquid of one of the extreme compartments. A plate of zinc was immersed in one and a plate of copper in the other, and the two were placed in metallic communication by means of the wires of a galvanometer.

Under these circumstances, when all the compartments were charged with dilute sulphuric acid ($\frac{1}{5}$ th acid), a deviation of the galvanometer needle was produced, which, as with an ordinary couple, gradually decreased. Upon consulting M. Becquerel's original memoir (*Ann. de Phys. et de Chim.*, tome xli, p. 21), I find the following tabular record of the results :—

Time of Immersion.	Deviation of Needle.
0'	63°
15	53
30	46

* "If then we could continually remove the alkaline and acid elements which are deposited on the plates we should annihilate the secondary current, and the pile would have a constant intensity, inasmuch, however, that the surfaces of the plates would remain also almost clear, and the action of the liquid on the zinc would be almost the same."

Upon re-commencing the experiment with the addition of $\frac{1}{10}$ th nitric acid to the dilute sulphuric acid in the copper compartment, the intensity of the current changed, but gradually diminished as in the following table :—

Time of Immersion.	Deviation of Needle.
0'	81°
15	73
30	65

Upon the substitution of nitrate of copper for the nitric acid, the results were sensibly the same. M. Becquerel varied these experiments in many ways, and with the aid of this apparatus arrived at the following conclusions :—

“ The maximum of intensity is obtained by immersing the copper plate in a solution of nitrate of copper, and the zinc in a solution of sulphate of zinc, *but there is also a diminution with time ; a little less rapid however than with an ordinary couple.*”

The following is the table of the last results :—

Time of Immersion.	Deviation of Needle.
0'	84°
15	72
30	68

The next paragraph of M. E. Becquerel's *Notice* I will give in his own words for fear of mistakes :—

“ Si l'on met de l'acide nitrique étendu dans la case zinc, un peu d'eau et d'acide sulfurique dans la case cuivre, et que dans la caisse A A' on n'emploie qu'un diaphragme, ou que l'on rapproche les deux diaphragmes de telle sorte que l'acide nitrique de la case zinc puisse passer lentement dans la case cuivre, afin d'augmenter la conductibilité, alors on peut, au lieu de rendre les intensités décroissantes les rendre croissantes pendant la première demi-heure ; cela dépend de la quantité d'acide nitrique versée dans la case zinc ; si l'on en met avec précaution, on peut arriver à obtenir un courant constant pendant une demi-heure ou une heure. Cette disposition du couple réalise donc pendant un certain temps le résultat que nous avons annoncé, c'est-à-dire détruit le courant secondaire ; car l'acide nitrique qui se trouve dans la case zinc s'empare constamment du cuivre de la solution, qui, après avoir traversé les diaphragmes, se dépose sur le zinc.

“ Ainsi ces expériences montrent la possibilité d'obtenir un courant constant en détruisant le courant secondaire.” (P. 440).*

* “ If we put diluted nitric acid into the zinc case, and a little water and sulphuric acid in the copper case, and that in the box A A, we only employ a diaphragm, or that we bring towards each other the two diaphragms in such a manner that the nitric acid of the zinc case may pass slowly into the copper cell, in order to augment the conductivity ; then we can, in place of rendering the intensities decreasing, render them increasing during the first half-hour : this depends on the quantity of nitric acid poured into the zinc cell ; if we put the nitric acid in with precaution we may be enabled to obtain

Now, most assuredly, the principles and results derived from these experiments are not the principles and results of "the constant battery:" they are not the principles and results of my battery, which M. Becquerel has nevertheless described as—"construit d'après les principes exposés précédemment," and, as one amongst other similar ones, "d'un usage plus facile et dont l'action est constante pendant un temps plus long." (P. 444).

The amount of *constancy* which M. E. Becquerel obtained, and the certainty with which he obtained it, are thus described in his original memoir:—

"Il m'est arrivée *plusieurs fois* d'obtenir une compensation telle que les déviations de l'aiguille aimantée étaient constants pendant une heure, avantage que l'on n'a jamais avec les piles ordinaires;"* and M. E. Becquerel, as we have just seen, says that "by adding nitric acid with *precaution* one may manage to obtain a constant current for half an hour or an hour."

With my constant battery, constructed with siphon tubes, as described by M. E. Becquerel, a steady current might be kept up, if necessary, for a week together, provided a proper supply of materials were furnished; and the amount of force set in action by it would be measured, not by some 60° of the galvanometer, but by the number of yards of platinum wire which it would fuse or render red-hot.

Even in the use of the diaphragm,† which might at first sight appear to be similar in the two constructions, there is direct opposition; for my object is to keep the two electrolytes which I employ perfectly separate, so that no portion of one may penetrate to the other, except in the process of electrolysis; while, according to M. Becquerel's principles, it is necessary that they should slowly mingle; and the diaphragm is placed to regulate the mixture. In his own words it is necessary "que l'on approche les deux diaphragmes de telle sorte que l'acide nitrique de la case zinc puisse passer lentement dans la case cuivre."

a constant current during half an hour or an hour. This disposition of the couple realises then, during a certain time, the result which we have announced, that is to say, it has destroyed the secondary current; *for the nitric acid which is found in the zinc case seizes constantly on the copper of the solution, which, after having traversed the diaphragms is deposited on the zinc.*

"Thus these experiments show the possibility of obtaining a constant current by destroying the secondary current."

* "I have been enabled several times to obtain such a compensation that the deviations of the magnetic needle were constant during an hour: an advantage which we never obtain with the ordinary piles."

† The passage of the voltaic current through diaphragms of bladder was well known to experimentalists before the publication of M. Becquerel's researches. The late Dr. Ritchie, amongst others, made frequent use of them; and in the *Phil. Trans.* for 1829, p. 363, there is a paper by that gentleman, in which "a small rectangular box divided into two compartments by a diaphragm of bladder" is described, which was used for the purpose of exposing the plates of a galvanic circle to two different liquids.

The purpose which the nitric acid is said to effect, is to me perfectly incomprehensible, namely, that of "taking up the copper of the solution, which after having traversed the diaphragms is deposited upon the zinc."

According to my experience, under these circumstances the deposited copper would be untouched, and the local solution of the zinc greatly promoted: but my present object is not to controvert M. E. Becquerel's statements further than is necessary to show that the principles which he has derived from his father's experiments are not the principles of my "constant battery."

M. Edmond Becquerel, however, seems to have some misgivings that the premises upon which he has founded his father's claim may not be deemed sufficient for the purpose; for he proceeds—

"Ainsi ces expériences montrent la possibilité d'obtenir un courant constant en détruisant le courant secondaire; c'est-à-dire en faisant plonger les lames dans des liquides différents. *Plus tard mon père a résolu complètement la question.*" (P. 440).*

This *complete* solution of the problem is comprised in the apparatus which M. Becquerel has named "chaîne simple à oxygène." It consists of two small glass vessels, of which one contains a concentrated solution of caustic potassa, and the other a solution of strong nitric acid. These two vessels are connected together by a bent glass tube fitted with porcelain clay wetted with a solution of sea salt. In each vessel is placed a small plate of platinum; and when a metallic communication is made between the two, a current is established of sufficient energy to cause the evolution of oxygen from the plate in the potassa, while the equivalent hydrogen is absorbed by the nitric acid at the opposite plate. When the communication is made between the plates by means of a galvanometer, the needle is deflected. This slow current will remain constant for twenty-four hours, or longer; but it ceases when the nitrate of potassa formed in the connecting tube crystallizes.

Now, it will be observed, that the porous diaphragm is here again employed, not to produce, as in my "constant battery," as complete a separation as possible between the liquids, but to regulate their mixture, which is essential both to the production of the primary current and, according to M. Becquerel, to the destruction of the secondary one.

"Il faut que les métaux qui composent chaque couple plongent dans des liquides différents, séparés par une membrane ou un diaphragme capable de laisser traverser peu à peu les liquides, cette disposition étant la seule par laquelle le courant secondaire produit

* "Thus these experiments show the possibility of obtaining a constant current by destroying the secondary current; that is to say, in causing the plates to be immersed in different liquids. *At a later period my father completely resolved this question.*"

par la polarization des électrodes puisse être détruit." (P. 442).*

M. E. Becquerel, however, allows that even this apparatus, which so completely solves the problem of the constant battery, is incapable of application.

"Ce dernier appareil résout complètement la question sous le point de vue scientifique, mais *pour les applications* le courant électrique n'aurait pas une énergie suffisante, car alors il faudrait de plus grands éléments et en plus grand nombre, ce qui serait assez coûteux."

He adds, "il s'ensuit que ces appareils, qui forment pile, ont une intensité d'action constante pendant un temps considérable: *cette condition n'est remplie que quand l'action est très lente.*" (P. 442).†

It is difficult, indeed, to conceive that M. E. Becquerel can be serious in attempting to trace an analogy between this arrangement, or any of the similar apparatus formed of "tubes en U," which his father has so successfully applied to the investigation "des actions lentes," and "the constant battery;" whose object is the generation of voltaic power, with a constancy and rapidity which promises to render it applicable not only to manufacturing but mechanical purposes.

The very source of the current in the "chaîne simple à oxygène," is different from that of "the constant battery." M. Becquerel, in his *Traité de l'Electricité* (tome iii, § 526), ascribes it to the slow and direct combination of the acid and alkali; but this explanation will not be deemed satisfactory, since Dr. Faraday has shown that the decomposition and polarization of the molecules of an electrolyte are essential to the establishment of a current. I have myself suggested an explanation of the undoubted phenomena of current and decomposition which take place, which is quite compatible with these conditions (*Phil. Trans.* 1840, p. 223). The origin of the force is, however, doubtless, to be found at the point of contact of the two liquids, and not at the electrodes; and I repeat, that in my "constant battery" nothing depends upon the contact and action of the two liquids upon each other.

The amount of force is also perfectly insignificant with regard to its application to the arts. It would be difficult to obtain by this arrangement a decomposition amounting to $\frac{1}{100}$ th of a cubic inch of

* "It is necessary that the metals which compose each couple are immersed in different liquids, separated by a membrane or a diaphragm capable of allowing the liquids to traverse little by little, this disposition being the only one by which the secondary current produced by the polarization of the electrodes can be destroyed."

† "The latter apparatus completely resolves the question in a scientific point of view; but for its applications, the electric current will not have sufficient energy, for then it would be necessary to have larger elements, and in a greater number, which would be expensive."

He adds, "It follows that these pieces of apparatus which form the pile have a constant intensity of action during a considerable time; this condition is only fulfilled when the action is very slow."

hydrogen and oxygen per minute, while with a constant battery of my construction it is easy to maintain a current equivalent to twenty cubic inches of the mixed gases in the same time. It is scarcely necessary to observe, that the difficulty of maintaining constancy of action is, of course, proportioned to the energy of the action.

It is this almost unlimited command of steady force which renders its application, either in the electrolytic or magnetic direction, possible to mechanical or manufacturing purposes: magnetic locomotive engines never could have been dreamt of without the solution of this preliminary problem.

M. E. Becquerel proceeds to say, "M. Daniell, *d'après les principes exposés précédemment*, a construit une pile dont nous allons donner la description, et qui est à présent généralement employée." (P. 442).*

Now I not only most emphatically deny that I was in any degree guided by these principles, but I assert that they were incapable of leading me to any such conclusion; as they, in fact, failed to lead M. Becquerel to the same.

In proof of this, I will briefly recapitulate the real principles of "the constant battery;" the different steps of the investigation which preceded its invention being all clearly stated in my papers in the *Philosophical Transactions*.

1. In the first place, I traced the origin of the decline and ultimate annihilation of the current in the common voltaic battery not to the evolution of acid and alkaline matter at the opposite plates, and the consequent establishment of a counter current by their mutual reaction, but to the deposition upon the conducting plate of a substantial coating of pure metallic zinc, the weight of which upon a platinum plate of three inches by one I have stated to be nearly twenty-nine grains. In consequence of this, zinc becomes opposed to zinc in the circuit, and all current is stopped.

2. To prevent this deposition of active metal upon the conducting metal, it occurred to me,—1st, to divide the portion of the electrolyte in contact with the generating plate from that in contact with the conducting plate by a porous diaphragm, by which the solution of zinc is prevented from reaching the copper or platinum; and secondly, to provide for the perpetual renewal of the surface of the conducting plate by the deposition of fresh inactive metal upon it, transferred by the process of electrolysis from the solution of a salt selected for the purpose and placed in the partition next to it, or precipitated by the secondary action of the hydrogen evolved upon it. By this contrivance, not only is the surface of the conducting plate perpetually renewed, but the opposing influence of the hydro-

* M. E. Becquerel proceeds to say, "M. Daniell, *after the principles precedingly exposed*, has constructed a pile of which we are now going to give a description, and which is at present generally employed."

gen during its evolution is removed, and the battery is at once rendered constant and its power greatly exalted.

As copper, from its comparative inactivity, its great conducting power, and its commercial value, is the only metal which is capable of being applied extensively for the construction of voltaic batteries upon a large scale, I constructed my cells of that metal; placing in contact with them a saturated solution of sulphate of copper in dilute sulphuric acid (eight measures of water, one oil of vitriol), separating this portion of the electrolytes from that which is in contact with the amalgamated zinc, and which is composed of the same diluted acid without the salt of copper, by a diaphragm of membrane or porous earthenware. Under this arrangement a part of the current is conveyed by the electrolysis of the sulphate of copper and a part by the dilute sulphuric acid: from the former the copper is directly deposited in that compact form which is now so well known from the formation of voltatypes, and by the latter it is thrown down in a less coherent form by the secondary action of the hydrogen; but both concur in the effects which have been previously described.

The only practical difficulty in this construction is the formation of diaphragms, capable, at once, of allowing the electrolytic action to proceed through them, and of perfectly preventing the mixture of the liquids on their opposite sides; the latter action being, it will be remembered, one of the essential principles of the batteries "pour les actions lentes," upon M. Becquerel's construction. If a portion of the solution of copper penetrates to the zinc side of the diaphragm it is decomposed, and the copper deposited upon the zinc producing strong local action and loss of the generating metal; for I have not been fortunate enough to find, with M. Becquerel, that the copper can be dissolved under such circumstances.

Such are the principles which I have derived from experiments, and which are capable of application to the construction of powerful constant batteries of different forms and materials. The form which I have preferred is that of hollow cylinders of conducting metal, with central rods of generating metal. This form is dependent upon another principle, which I also worked out from experiments upon the diffusion of the force of a small generating surface over a large conducting surface. These have also long since been published in detail in the *Philosophical Transactions*, but they are not alluded to in M. E. Becquerel's *Notice*. This cylindrical arrangement is one of great capacity, and admits of the employment of large quantities of the electrolytes which are required for energetic and long-continued action.

But, of course, the principles of the construction are independent of form and materials, and are capable of application to flat, square and equal surfaces of the two metals, as well as to concentric arrangements. They admit also of the employment of different metals and of different electrolytes. They are not changed by placing the zinc on the outside instead of the inside of the copper,

nor even by altering the name of the "constant battery" to that of the "sustaining battery."

I will embrace this opportunity of observing that M. Becquerel, in his *Traité de l'Electricité* (tome v, livre xiv, p. 195), has inadvertently committed an error affecting the history of "the constant battery," in giving priority to Professor Grove's experiments with the nitric acid battery over mine. He describes that gentleman's construction, and then proceeds, "*Les choses en étaient là lorsque M. Daniell a repris la question, qu'il a analysé avec sagacité. Ses recherches l'ont mis à même de doter la science d'une pile construite d'après les principes précédentes, et qui est aujourd'hui généralement adoptée dans les expériences de physique.*"*

It is only necessary to recall the data of my first paper upon the subject in the *Philosophical Transactions*, viz. February 11th, 1836, and that of Professor Grove's communication to the French Academy, I believe, April 1839. Professor Grove has never spoken of his battery but as the further application of principles which I had previously deduced.

In conclusion, I cannot but express my regret that the filial piety of M. Edmond Becquerel should have betrayed him into an act of injustice by preferring a claim for his father which is totally unsupported by the facts of the case, and from which his well earned reputation can derive no permanent extension.

I remain, dear Sir, very faithfully your's,
King's College, London, March, 1842. J. F. DANIELL.

On the Sustaining Voltaic Battery: in reference to some Observations of Professor Daniell, in the April Number of the "Philosophical Magazine."† By F. W. MULLINS, Esq., M.A., &c.

TO WILLIAM STURGEON, Esq.

DEAR SIR—As the conductor of a magazine, justly denominated the "Guardian of Experimental Science," I trust you will kindly make room for a few observations called forth by the following paragraph contained in the letter of Professor Daniell, "On the Constant Voltaic Battery," published in the *Philosophical Magazine* for March.

After setting forth the differences between the principles of the "constant battery," and M. Becquerel's voltaic combination discovered in the year 1829. Mr. Daniell adds: "But of course the

* "*These particulars were there when Mr. Daniell again took up the question, which he has analyzed with sagacity. His researches have endowed science with a pile constructed after the preceding principles, and which is generally adopted in philosophical experiments.*"

† The preceding article is that alluded to. Professor Daniell's voltaic arrangements and experiments are fully described in vols. 1 and 3 of the *Annals of Electricity*, &c.—EDIT.

principles of the construction are independent of form and materials, and are capable of application to flat, square, and equal surfaces of the two metals, as well as to concentric arrangements. They admit also of the employment of different metals, and of different electrolytes. *They are not changed by placing the zinc on the outside instead of the inside of the copper, nor even by altering the name of the constant battery to that of the sustaining battery."*

Now, as it is quite clear that the latter part of this paragraph distinctly refers to "Mullins's Sustaining Battery," that being the distinguishing appellation applied to it by me, and *borne by it alone*, and as the inference to be drawn from the entire sentence is, that the principles of the "constant" and "sustaining" batteries are not only identical, but that the latter arrangement has been borrowed from the former—I cannot permit such an impression to acquire further publicity, without at once entering my protest against the correctness of such a conclusion, as well as against the mode in which it has been put forward.

I find a similar misstatement in *Walker's Electrottype Manipulation*, but I have not thought proper to notice it, nor should I do so on the present occasion, did I not find Professor Daniell himself using language which might be understood to sanction inuendos incapable of proof, and utterly groundless.

Sir, I formally and distinctly deny that the principles of the "sustaining" are the same as those of the "constant voltaic battery;" and I cannot but consider it *extremely strange* that Professor Daniell should hazard such an opinion in a letter written professedly for the purpose of showing (as in the case of *Becquerel's* battery and his own) that voltaic combinations, although constructed of *exactly similar materials*, with the *similar* design of obtaining *constancy* of effect, may yet have *no one principle* in common!!

But as facts are stubborn things, and as I like to deal with *facts*, and not *opinions*, let me first state, in Professor Daniell's *own words*, what *are* the principles of *his* combination, and then explain those of the other.

First then, Mr. Daniell declares his "constant" battery to be constructed "on the principle of a *central* disposition of the active metal, with regard to the conducting surface,"* and he carries out this principle in his battery, by the employment of a rod of *east* zinc, *amalgamated* and suspended in the *centre of a circle of copper*, the said rod being, according to his detailed measurements, half an inch in diameter, and the copper cylinder surrounding it, three and a half inches; which the Professor states, "experience has proved to afford the *most advantageous* distance between the generating and conducting surfaces."† He thus interposes, it would appear, *by preference*, an imperfect conductor of one and a half inches interval between the metals; this interval, of course, *continually increasing*

* See his *Chemical Philosophy*, page 438, sec. 737.

† See latter part of sec. 737, in *Chemical Philosophy*.

in *proportion* to the *destruction* of the *central zinc*, and consequently, causing additional retardation of the current. Yet he lays it down as an incontrovertible position, that *such* an arrangement develops more electric energy, and is more constant in its effects, than equal surfaces of similar metals in any other mode of combination. Now, in the "sustaining battery," these principles of Daniell's are *directly* opposed by the nature of the arrangement, in which there *cannot* be any such thing as "central action," and in which retardation is *diminished* by *approximating* the metallic surfaces as much as possible: so that, in *place of one inch and a half*, as in Daniell's arrangement, they shall be no more than a *quarter*, or at most, *half an inch asunder*: a principle at variance with Daniell's; grounded on the results of careful experiments, and supported by the distinguished authority and varied investigations of Davy and Faraday, Hare, De la Rive, and many other eminent philosophers.* Secondly, Professor Daniell advocates the use of an *acidulated* solution in contact with the zinc, which he declares it is requisite to *amalgamate*, in order to avoid *local action*; whereas in the sustaining battery, *no acids are used*, alkaline solutions being employed as far preferable: the zinc is *not amalgamated*, there being *scarcely any local action*: *rolled* instead of *cast* metal is advantageously used, and the general chemical effects are different. Again, Professor Daniell asserts another of his principles to be, "that of *perfectly preventing* the mixture of the liquids on the opposite sides of the diaphragm employed," such mixture "*producing strong local action*," &c.; whereas, in the sustaining battery, a diametrically opposite principle was from the very first brought into play, by the employment of the very thinnest membranes that could be procured;† it being found that the mixture of the fluids, within a certain limit, *increased considerably the effect*; this mixture arising in a great degree, from the action of "endosmose;" which, according to M. Dutochet, has most probably an electric origin, and, according to my observations, unquestionably promotes electrolysis; and this principle, *the very opposite of Daniell's*, was further carried out by the employment of *white silk* as a diaphragm, which would be superior to every other I know of, but for the disposition of the precipitated copper to collect within its interstices.

Having thus briefly, but I trust clearly, set forth the principles upon which the sustaining battery was constructed, and shown that they are *altogether opposed* to those claimed by Professor Daniell, I leave it to him and M. Becquerel to settle the question of originality as best they can; for my part, I make no claim to originality in regard to the principles which governed the construction of the bat-

* We have, for many years, employed batteries in which five pairs occupied one inch only of its length.—EDR.

† Those membranes were abandoned solely in consequence of their liability to injury; wooden diaphragms have been substituted by me, which admit of a certain mixture of the liquids, and are extremely durable.

tery : principles long established by the researches of distinguished men at home and abroad : but I *do* claim the merit, whatever it may be, of the conclusions drawn from those principles as developed in the particular voltaic arrangement alluded to.

It only remains for me to add, that even supposing the principles of construction of the "sustaining" battery *were* identical with Daniell's (which they *are not*), it would be extremely unfair to argue or assume that Mr. Daniell's researches furnished the clue to the plan of the other combination ; for I unhesitatingly assert that for years before Daniell's constant battery was known, every reader of the *Annales de Chimie et de Physique*, tome 41 (myself amongst the number), must have been fully aware of Becquerel's discovery in the year 1829, of the possibility of obtaining an equal current for a comparatively long period, by the employment of two fluids : one a solution of a salt of copper, the other diluted sulphuric acid, with a membrane (*baudruche*) interposed.* In this combination the *true principle* of a sustaining battery was, beyond question, established ; and although by reason of certain *mechanical* defects in the construction of Becquerel's batteries, *as admitted by himself*, the energy was not sustained, so long as it *now* is, by *improved* mechanical arrangements, I do insist that to Becquerel is *justly* and *fairly* due the honour of being the *first promulgator* and *constructor* of a constant voltaic combination of elements or materials similar to those subsequently employed by Daniell, and with the exception of the *alkaline solution*, similar also to those employed by me.

One word more, and I have done. Mr. Daniell sets forth in the letter alluded to, the "real principles of the constant battery ;" and, inasmuch as he claims originality for those principles, and hints pretty broadly that such original principles have been made use of and applied by *others* in combinations claimed by them as *equally* original, it may be well to give, *in his own words*, his statement of the first of those principles, and answer him in the words of another, whose scientific reputation fully equals his own. Mr. Daniell says, in the first place, "*I traced the origin of the decline and ultimate annihilation of the current in the common voltaic battery, not to the evolution of acid and alkaline matter at the opposite plates, and the consequent establishment of a counter current by their mutual reaction, but to the deposition upon the conducting plate of a substantial coating of pure metallic zinc ; in consequence of this, zinc becomes opposed to zinc in the circuit, and all current is stopped.*" Dr. Roget, in his *Treatise on Galvanism*, published in 1832,† page 14, after describing the effect produced by the contact of the metals in the circuit, proceeds as follows :—"The disengaged hydrogen will *now* appear upon the surface of the *copper* in place of the zinc,

* Our readers may expect a translation of Becquerel's paper in an early number.—EDIT.

† See *Treatise on Galvanism* in the "Scientific Treatises of the Society for Diffusion of Useful Knowledge."

without, however, apparently affecting the copper. In process of time, indeed, when a considerable proportion of sulphate of zinc has been dissolved in the fluid, *the quantity of disengaged hydrogen is found to diminish*, and a thin *film*, composed partly of *metallic zinc*, and partly of filaments of oxide of zinc, is *deposited on the surface of the copper : as soon as this happens the galvanic action ceases.*"

I need not here advert to Sir H. Davy's distinct statements on the same subject, for I believe they are too well known to need repetition: suffice it to say, that the deposition of *metallic zinc* upon the *conducting surface*, and other causes of the decline of voltaic energy were well understood for years before Professor Daniell produced the constant battery.

Mr. Daniell proceeds to say, that "his next principle was to prevent the deposition of active metal upon the conducting metal, by dividing the portion of the electrolyte in contact with the zinc, from that in contact with the copper, by a porous diaphragm, by which the solution of zinc is prevented from reaching the copper." Now, if deposition of zinc were the *sole* or *chief* cause of the decline of voltaic energy, *one fluid, so divided*, would have answered every purpose; but this was *not so*, therefore it appeared requisite to introduce the salt of copper. But it so happens, that when a cupreous salt is employed, *though there should be no diaphragm*, there is *no deposition of zinc on the conducting metal*; and I have found, and M. De la Rue and others have *proved*, that the energy of the battery *so charged*, may be sustained for a *long period without any diaphragm*; therefore, upon the *ground of preventing deposition of zinc*, Mr. Daniell's protection of the negative metal was *unnecessary*, and therefore that part of his principle *cannot* be sustained. For the above reasons I did *not* adopt a diaphragm "for the purpose of *preventing a deposit of zinc upon copper*," but for the *directly opposite purpose of preventing a deposition of copper upon zinc*.

Regretting that I have been compelled to thus defend myself from most unwarrantable insinuations, as well as support my just title to whatever merit may attach to a form of voltaic combination which, for *sustained* as well as *powerful* effects, is now pretty generally appreciated; and assuring Professor Daniell that I have no wish to deprive him of any credit to which he may be *fairly* entitled; or to insinuate that he read, or had read to him, or heard of M. Becquerel's researches, as published in the *Annales de Chemie*, tome 41, or subsequently, either in the *Annales*, or any other scientific work, foreign or domestic—I feel that I have stated quite sufficient to prove that I am no imitator of the Professor, and shall, therefore, not be again induced to notice such uncalled-for attacks, no matter from what quarter they may emanate.

I am, dear Sir, your's faithfully,

Bruxelles, April 9, 1842.

F. W. MULLINS.*

* A full description of Mr. Mullins's "sustaining battery" may be seen in vol. 1 of these *Annals*.—EDIT.

Memoir on the General Principle of Natural Philosophy. By M. G. LAME, Professor at the Polytechnic School.*

THE discovery of ether is due to the physical theory of light; the idea of luminous undulations, defended by Euler and Young, has had a definite triumph by the discoveries of Arago and of Fresnel; since the fruitful theories of the latter, the more recent works of MM. Hamilton and Lloyd, Brewster, Babinet, Seebeck, and the mathematical researches of MM. Cauchy, MacCullagh, and Neumann, it is no longer possible to doubt the existence of ether. The physical theory of electricity, excited by the discovery of CErsted and the works of Ampere, all conjoin at the present day to make all the electrical phenomena depend on the mutual actions of ether and the ponderable matter; and it is evident that to this preconceived idea are due the greater parts of the discoveries of MM. Becquerel, De la Rive, Faraday, and Savary. In fine, the physical theory of heat, since the works of MM. Melloni and Forbes, is in vain defended against the idea of vibrations.

In resuming the subject, the general principle, towards which the three partial theories of natural philosophy at the present day converge, attributes to ether, to its own repulsions, and to the actions which the ponderable matter exercises on it, all the phenomena which depend on these theories. The propagation of the vibrations of the etherial fluid gives light, and all the radiations. The increase or diminution of the masses of ether, which form the atmospheres of ponderable atoms produces electricity and chemical phenomena. In fact the vibratory movement of these atmospheres gives heat.

This principle contains in itself all the hypotheses elaborated by the philosophers who occupy themselves on the different departments of philosophy; it embraces and explains sufficiently the phenomena of all classes. This generality does not show, without doubt, its real existence, but it gives to it, at least, as much of certainty as to the original ideas of modern discoveries, since these ideas are found to be comprised in the complete expression of the principle which it contains. The consequences deduced from this principle present themselves with a probability then sufficient to attract the attention of philosophers.

One of these consequences is the existence of a pressure exercised by the ether on itself, on all other bodies, and in the interior of all the millions of other ponderables. The non-manifestation of this pressure results from the fact that there is no body in existence entirely deprived of ether; and that because all the particles of the fluid communicate freely among them, by the layers which separate the ponderable atoms, whose complete contact nowhere exists. This pressure ought to surpass in greatness the cohesion of all the trans-

* *Comptes Rendus*, Jan. 3, 1842.

parent solids, for it is that which maintains them : as the atmospheric pressure maintains the liquids which evaporate in a vacuum. But again, if we take a glimpse at any kind of barometer which can give us the exact amount of this pressure, there exists the means of proving its variation ; such is, amongst others, the measure of the co-efficient of the dilatation of the gases. If the pressure of the ether is greater at the present day than at the epoch when M. Gay Lussac measured it for the first time, in an exact manner the dilatation of the gases, the two fixed points of the thermometer, ought to approach each other ; which explains in a very simple manner the non-agreement of the co-efficient measure at two epochs. In fact, after the actual memoir :—

1st. *The tension of the vapour of water, measured at a determined temperature, is only the excess of its total elastic force on the pressure of the ether in vacuo ; and if the pressure augments, the vapour ought in reality to be raised in temperature, inasmuch as its measured tension remains constant. That is to say, that the fixed point for the ebullition of water, under a barometric pressure of $0^{\text{m}}.76$, ought to be higher.*

2nd. *The fusion of a solid takes places when the resultant of the interior repulsive forces surpasses a little the pressure of the ether ; that is to say, that the degree of the fusion of a solid, under the pressure of the ether, is analogous to that of the ebullition of a liquid, under the atmospheric pressure. Then, if the pressure of the ether is augmented, the temperature of melting ice ought to be raised.*

The ascending variation of the fixed point of melting ice ought to be much greater than the variation in the same direction of the fixed point of ebullition. From some experiments of verification (in which I have been assisted by M. Cobart), I have proved that the tension of the vapour of water, at the temperature of melting ice, is still at the present day $5^{\text{m}}.$, as M. Gay Lussac found it at an epoch but little removed from that in which he measured the dilatation of the gases. We may, then, say at the present day, as then, that the temperature of melting ice is that where the tension of the water is $5^{\text{m}}.$ This concordance proves that the resultant of the repulsive forces in the ice, and the elastic force of the vapour of water, at the temperature in which ice melts, preserve a small constant difference, whatever be the variation which this temperature may undergo through a change in the pressure of the ether.

However, it is sufficient to consult the table of the tensions of the vapour of water, in order to see that if these tensions represent the excesses on the pressure of the ether, an augmentation in this latter pressure ought to raise the temperature at which the measured tension is $5^{\text{m}}.$; that is to say, the zero of the thermometer, of a quantity of from seventy to eighty times greater than the correspondent variation of the point of ebullition, or of the temperature at which the measured tension of the vapour of water is of $760^{\text{m}}.$ Thus the two fixed points of the thermometer ought to approach each other.

It is to be presumed, then, that the non-accordance of the number found, during twenty-five years, by M. Gay Lussac, to express the co-efficient of the dilatation of gases with that found at a later period by M. Rudburg, and verified by M. Regnault, results entirely from variations of this nature. The difference of the two results explain themselves, by admitting that the pressure of ether has undergone on the earth, in a quarter of a century, an augmentation equivalent to a pressure of eight or nine-tenths of a millimeter in height of the mercury : a small increase, but which will suffice, however, to bring nearer to each other the two fixed points of the thermometer by $2\frac{1}{4}$ degrees, taken on the old divisions ; for by dividing the total dilatation of the air between the two fixed points, obtained by M. Rudberg by 97.75, and not by 100, we re-obtain the number given by M. Gay Lussac.

Professor STEINHEIL on a simple Method of Showing the Existence of Induced Counter-Currents by Direct Experiment. From the *Gelehrte Anzeigen*, Munich, Dec. 1, 1837. Translated by W. LETTSOM, Esq.

BETWEEN the poles of a compound horse-shoe steel magnet let there be adapted a rotating anchor of soft iron, surrounded with covered wire. The ends of this wire dip down into mercury cups separated by a partition, the cups themselves being brought into connection with the terminal wires of a battery. Between one of the above cups and the battery a multiplier is to be inserted.

As soon as the battery is set in action, the anchor between the poles begins to rotate ; and let us suppose the needle of the multiplier to be deflected at the same time to the right. Now break off the connection with the battery, and spin round the anchor by the application of some mechanical force in the same direction in which it was previously impelled by the battery : the needle of the multiplier will be deflected in the opposite direction ; that is to say, to the left, and the amount of this deflection increases with the rapidity of the motion imparted to the anchor.

By the rotation of the anchor, caused by the battery, there are, therefore, currents of *opposite signs* called forth, and in which the counter-current is stronger the more rapid the motion is, or the greater the strength of the magnet is, and from which there must naturally result a proportionate, and not an accelerated velocity ; and as a further proof of this, it may be added, that the velocity of the rotating apparatus is increased the very instant that the magnetic power is reduced by the application of an anchor—that is to say, while we weaken the magnet, we reduce the power of the induced counter-current.

This experiment appears so convincing, and so irreconcilable with Moser's views on electro-magnetic machines—namely, that the

action is not called into play the very instant the circuit is completed, and that it is the rapidity in the succession of these impulses which limits the effect (see Moser, *Repertorium der Physik*, vol. i, Berlin, 1837, p. 281), that we cannot but hold that philosophy to be in error; and are led to think, with Jacobi, that the absence of the power once expected from these machines, arises from the counter-currents induced in the copper helices by the motion of the rotating magnets.

Experimental and Theoretical Researches in Electricity, Magnetism, &c. Seventh Memoir. By WILLIAM STURGEON, Superintendent and Lecturer of the Royal Victoria Gallery, Manchester; formerly Lecturer on Experimental Philosophy at the Hon. East India Company's Military Academy, Addiscombe; &c.

SECTION I.

On a Peculiar Class of Voltaic Phenomena.

THE great interest excited by the developement of those facts which show the various means by which modifications of chemical action on metallic bodies by acid menstrua are produced, having induced the British Association to grant pecuniary aid to Professor Schöenbein to enable him to pursue those experimental enquiries in which he had previously been so successfully engaged on this subject, it cannot be presuming too much to suppose that the Association would be desirous of becoming acquainted with every fact connected with so interesting a branch of physical science. And as I think it possible that Professor Schöenbein may not be in possession of some particular facts of this class, which were discovered several years ago, and as the theoretical views which accompanied the publication of those facts appear to me to be applicable to every phenomenon of this peculiar class hitherto made public, I now venture to introduce them to the notice of the British Association,* simply as adscititious data, independently of which any historical view of the discoveries which have been made in this class of phenomena, and of the theoretical notions which have been advanced for their explanation, must necessarily be incomplete.

347. The experiments to which I allude were published in the year 1830, in a pamphlet, a copy of which accompanies this paper; and as the experiments are clearly described, and the theoretical views which I then entertained respecting them are unequivocally stated in the original, I cannot do better in this place than to refer to them as they stand in a note commencing at page 77 of that work.† This theory of the electro-chemical action of the simple metals on acid and other solutions was republished in the "Annals of Electricity,"

* The first section of this Memoir formed the substance of a paper which was read at the Glasgow Meeting of the British Association, 1840.

† The reader is referred to vol. I of the *Annals* for a perusal of the theory alluded to.

&c., for October, 1836, commencing at page 11; and a more extended application of that theory commences at page 17 of the same number.

348. Having thus directed the attention of the Association to the character of the phenomena which I had discovered and published more than ten years ago,* it will appear obvious, by comparing them with the extensive series of facts which have subsequently been developed by various experimenters, that the whole are belonging to one and the same class of phenomena: that they have an electric origin: and are particular cases of, and easily traceable to, the same general laws of electro-chemical action which I have so clearly portrayed in the pamphlet already referred to.

349. We must not, however, overlook the labours of Bergman and Keir, the latter of whom has given a long list of discoveries of this class of facts,† all of which are as highly important as any that have been subsequently developed. Mr. Keir clearly describes some of those facts which have appeared as novelties within the last few years; and has shown that iron acquires that *altered*‡ state by the action of nitric acid, which Sir John Herschell met with in his experiments, and has called the *prepared* || state; and what Schöenbein and others call the *peculiar*, or the *inactive* state.

350. The fact, also, that iron in certain conditions will not precipitate copper from its sulphate and other solutions, as recently observed by M. Schöenbein, was one of the many beautiful phenomena discovered by Keir, and clearly described in the *Philosophical Transactions* for 1790. Keir describes a number of similar experiments on the solutions of various metallic salts, and the phenomena in every case are obviously of the same class, and easily shown to be of electrical origin.

351. I have looked very attentively to the experiments of Herschel, Schöenbein, Andrews, Noad, &c., and I have repeated and varied many of them, and instituted others to a considerable extent, in order that I might be enabled to ascertain how far those theoretical laws which I had set forth would become applicable to the phenomena by the severest test which they could present. The electro-chemical phenomena developed by the action of two distinct metals, or by the action of two pieces of the same kind of metal, in acid or in alkaline solutions, are so easily traced to voltaic electricity, that no difficulty whatever is presented to their explanation. Sir Hum-

* Sir J. Herschel's paper on this subject, which is next on record, is dated August 19, 1833. See *Annales de Chemie*, or *Philosophical Magazine*, for October, 1837.

† *Philosophical Transactions of the Royal Society of London*, 1790, p. 353; also *Hutton's Abridgment of do.*, vol. xvi, p. 694: and *Annals of Electricity*, vol. v, p. 427.

‡ Kier calls that iron which is active in nitric acid, *fresh iron*; and that which has become inactive, *altered iron*. See *Annals of Electricity*, vol. v, p. 439.

|| *Philosophical Magazine*, for October, 1837, p. 330.

phrey Davy, in his Bakerian Lecture for 1826, has produced several interesting facts of this class, with one kind of metal only: and several others are described in my pamphlet already alluded to, and are similar to some of those which Professor Schöenbein has met with, and enquired for an explanation.*

352. It may probably appear singular that I should compare Keir's experiments on iron, in which only one metal is employed, with another in which two are made use of. I therefore wish it to be understood, that on the surfaces of those simple metals which are *active* in any acid solution, the particles are as decidedly in different electric conditions as any *two* distinct metals can possibly be, and act voltaically on the liquid accordingly. Hence in an electrical point of view, each simple metallic body presents different electric surfaces, which are equivalent to a voltaic combination of two or more distinct metallic bodies. This once admitted, the *inactivity* of a metal, after some minutes immersion in an acid solution, follows as a matter of course, for the very same reason that two distinct metals become inactive after being some time in an active voltaic condition. Their *relative* electric conditions have suffered a change, and they become inactive in that particular liquid, though they would still be active in another; and a new pair of metals, or a new piece of a *single metal*, would also be active in the old liquid.

353. There are several experiments described, both in the Bakerian Lecture for 1826, and in my pamphlet, in which the metals employed have changed their electrical characters, and the currents change their directions accordingly. Iron is very susceptible of these electrical mutations. A combination of two pieces of iron, as nearly alike as they can possibly be made to appearance, will sometimes exhibit several mutations of this kind before they finally cease to produce a current. In their first condition they are active, and A is positive to B; in a short time they are neutral to each other; afterwards B becomes positive to A, and becomes more and more so, till the needle indicates the maximum of action. The action again declines, and again they pass through the neutral point, and again A becomes positive to B. And after several mutations of this kind, the needle indicates that the two pieces are so nearly alike in their electrical characters that they are unable to produce an appreciable current. But *agitation* in the old liquid, or *immersion* in a new one, again brings them into a state of electrical activity.

354. Similar electro mutations, and final neutrality, take place on the surfaces of single pieces of metal.

355. There are different causes for metallic bodies changing their electrical characters by voltaic action, all of which will be found in the attachment of one or more of the constituents of the liquid employed. Hence the attachment of oxygen, nitrogen, metallic particles, &c., to metallic surfaces, will change the electrical character of them, and in many cases completely neutralize them, as regards particular li-

* *Philosophical Magazine*, for February, 1837, p. 133—4—5.

quids, though they may still be sufficiently active in other liquid bodies. Hence the voltaic energies on the surface of gold and platinum are too feeble to decompose any of the simple acid solutions, though sufficiently powerful to become active in their well-known solvent.

356. The particles of the liquids, and of their constituents, have also their electrical influence in all voltaic actions, and are electrically modified as decidedly as the surfaces of solid metallic bodies.

357. In all cases where simple metals are first active and afterwards inactive in acid solutions, it should be borne in mind that before they become inactive they are absolutely in *metallic solutions*, by the dissolution of a portion of their own substances. Hence in those experiments by Bergman and Keir, the cases were precisely alike, whether the iron was immersed in an already prepared metallic solution, or immersed into nitric acid only. And the more modern experiments with bismuth, &c., are of precisely the same character; and their inactivity, produced by the re-attachment (precipitation) of their own previously dissolved metallic particles, which give a new electrical character to the surface, and thus reduce the voltaic energies too low to remain any longer active on that particular liquid, though still sufficiently active to operate on a different one. Hence the cause of their brightness during their *apparently* inactive state: for in reality no metal is perfectly inactive when a fluid is presented to it suitable for a display of its voltaic energies.

358. With respect to other phenomena in the display of which only one individual piece of metal is employed, as first shown by Keir, they remain without even an *attempt* at explanation by any of the philosophers under whose notice they have appeared. Sir John Herschel pronounces them as of an "extraordinary character;"* Professor Andrews, after giving some very satisfactory explanations of several phenomena, acknowledges that he "can offer no explanation of most of the particular facts which have been described;"† and Professor Schöenbein has not yet made public any explanation of them whatever.‡

359. Under these circumstances it would be impossible to form any correct idea of the nature of the theoretical views on this subject which Professor Schöenbein will offer to the British Association at the Glasgow meeting. If, however, that philosopher should honour me with an attentive perusal of those facts and theoretical explanations to which I have already solicited the attention of the Association, it is possible I think that he will be enabled to find an easy and natural explanation to every phenomenon hitherto developed in this peculiar class, the whole of them being traceable to those laws,

* *Philosophical Magazine*, for October, 1837, p. 333.

† *Ibid.* for April, 1838, p. 311.

‡ An abstract of Herschel's and Schöenbein's experiments will appear in our next number.—EDIT.

which, though many years before the public, he may probably till now, never have had an opportunity of becoming acquainted with.

360. I have shown that zinc, which is the most energetic metal in dilute sulphuric acid, may have its action either *partially* or *totally* annihilated in that liquid by the contact of copper wires;* and I have shown that amalgamated zinc is perfectly inactive in solutions of sulphuric acid,† but that it is still active in solutions of nitric acid.‡ These are cases in point, and the latter is of precisely the same character as those shown by the iron in Keir's experiments. In one liquid it is *inactive*, in another it is *active*.

361. The explanation in these and in all similar cases, is referable to the difference of voltaic action in the solid and fluid bodies employed.

WILLIAM STURGEON,

Royal Victoria Gallery for the Encouragement of
Practical Science, Manchester.

September 11, 1840.

NOTE.—The contents of this Memoir were reported in the volume of the "Proceedings of the British Association for the Advancement of Science, for 1840," in the following manner:—

On a *peculiar Class of Voltaic Phenomena*, by Mr. Sturgeon.—
"The author directed attention to some experiments published by himself in 1830, and to his theory respecting the electro-chemical action of the simplest metals on acid and other solutions. He stated that the fact of iron not precipitating copper from its sulphate and other solutions, as recently observed by Professor Schöenbein, was one of the many beautiful phenomena discovered by Keir, and published in the *Philosophical Transactions* for 1790."

SECTION II.

On the Theory of Voltaic Electricity.

362. Perhaps there is no individual branch of physical science that has exercised the ingenuity of philosophers to a greater extent, respecting the *modus operandi* whence emanates its diversified phenomena, nor has any one been more productive of diversity of opinion, as a theoretical topic, than voltaic electricity, from the first days of its discovery to the present time: and what may, perhaps, be considered still more remarkable, no theoretical discussion has been less successful in uniting the opinions of scientific men, so as to give a general sanction to any theoretical views that have hitherto been taken, from whatever quarter they may have emanated, than that which has proceeded from this subject, and which has now been continued through a long series of years, and still remains as undecided as at first.

* See my *Experimental Researches, &c.*, experiments A and B, p. 78—79.

† *Ibid*, p. 41—42—74—76—77.

‡ *Ibid*, p. 43.

363. Under these circumstances, therefore, any attempt that can be made, in this place, to elucidate the principles of a science in which the efforts of the highest minds have failed to produce a satisfactory explanation, will necessarily appear under great disadvantages, with every probability of suffering the common fate of its predecessors. But as there are *some* points on which philosophers entertain the same opinion, there is still a possibility, at least, though the attempt may be viewed in the character of a *forlorn hope*, of simplifying *some* others, so as to be sufficiently comprehended to gain general assent, and thus advance us one step farther into the apparent intricacies of the theory of voltaic electricity.

364. I believe that there is no difference of opinion, at the present day, respecting the identity of the agency in common electricity, and that which is productive of the phenomena in that branch of physics now under contemplation: hence it is that we are not misunderstood by the term *voltaic electricity*, when represented as a peculiar branch of electrics. The generality of writers, however, on these matters, make no distinction whatever between *voltaic electricity*, and *galvanic electricity*; or, in other words, between *voltaism* and *galvanism*, although the sources from which the phenomena emanate are as distinct from each other, as that of the phenomena of ordinary electricity is from either of them. Why this practice is not yet abandoned would not, perhaps, be easily determined; but it must proceed either from an ignorance of their distinction, from an unwarrantable proneness to confound the one with the other, or from a fear of stepping counter to custom, and thus avoiding the imputation of pedantry.

365. I am of opinion, however, that the omission of even a *pointed* distinction in "*voltaic electricity*," and "*galvanic electricity*," by writers of the present day, is not only a dereliction of duty towards their readers, who purchase and read their works under the impression of finding in them expressions of the clearest ideas that the science is capable of admitting, but the practice is fraught with the seeds of misconception and error. Therefore, whatever risk may be run on the score of pedantry, it is quite time that the practice of confounding the two sources of electric action with one another were laid aside, and that distinction resorted to which alone can lead to clear and unequivocal views, and facilitate the association of ideas with the true character of the source of action.

366. In the preface to Aldini's "*Account of the late improvements in Galvanism*," published in the year 1803, we find that the distinction between *voltaism* and *galvanism* would appear to be very clearly pointed out:—"A just tribute of applause has been bestowed on the celebrated Professor Volta, for his late discovery, and I have no desire to deprive him of any part of that honour to which he is so justly entitled; but I am far from entertaining an idea that we ought on this account, to neglect the first labours of Galvani. Though these two philosophers pursued different routes, they concurred to throw considerable light on the some points of science: and the

question now is, to determine which of them deduced the most just consequences from the facts they observed, and then to ascertain whether the facts established by Galvani led to the theory of Volta, or whether those discovered by Volta are connected with the theory of Galvani. For my part, I am of opinion that these *two* theories may serve in an eminent degree to illustrate each other.

"Last year Professor Volta announced to the public the action of the *metallic* pile. I here propose to exhibit, according to the principles advanced by Professor Galvani, the action of the *animal* pile." And again, "In the first part of this work, I shall exhibit the action of galvanism *independently of metals*, and explain some of its general properties."

367. It is singular enough, however, that Aldini, who was the nephew of Galvani, and who laboured hard to show the distinction of the sources of galvanic and voltaic electric action, should eventually merge the one in the other, and call a series of "eighty plates of silver and zinc," a "*galvanic pile*."*

368. The distinction between the sources of galvanism and voltaism, is very marked, and susceptible of clear and unequivocal definition. The former being either a natural or an artificial association of *animal* matter, whether alive or dead: whilst the phenomena of voltaism emanate from associations of metals and other inorganic bodies. Galvanism, therefore, comprehends all those phenomena developed by *animal* electricity, and voltaism comprehends those developed by the simple contact of inorganic bodies, whether solid or fluid.

369. The electrical organs of the Torpedo and Gymnotus Electricus, and the muscular and nervous systems in all animals, are natural associations of animal matter, constituting sources of *galvanic* electrical action; and are as decidedly galvanic arrangements as are the artificial piles of muscle and brain first arranged by M. La Grave, and shown to the Galvanic Society of Paris: and afterwards operated with by Aldini, and various other philosophers.

370. In most of those electrical arrangements of inorganic matter hitherto formed, one, at least, of the bodies employed, has been a metal, and an association of *two* metals is invariably employed in every arrangement from which much action is derived. But it must not be considered from the hitherto invariable practice of employing metals in voltaic electrical arrangements, that electric action is not derivable from associations of non-metallic bodies; for it is well known that charcoal will supply the place of a metal, and it may easily be shown that the contact of liquids alone will produce electric action. These and all other associations of inorganic matter are sources of *voltaic electricity*.

371. With respect to the mode of action in voltaic associations generally, since the whole of that action is acknowledged to be electric, the principles of electricity, properly applied, are conse-

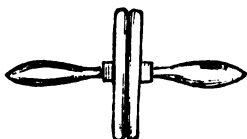
* See Aldini's work, page, 218.

quently those to which this *modus operandi* must be traced, and to which alone it can be attributable. The theoretical views of electricity which I have taken, are clearly described in my first memoir (40—87),* and are those alone which it will be necessary to apply on the present occasion, in forming the basis of the theory of voltaic electricity.

372. In the explanation of the principles of this theory I shall commence with the simplest case, hoping that the reader will bear in mind all that I have said, in my first memoir, respecting the unequable distribution of the electric fluid amongst the bodies composing the surface of the earth, and as far within its body as has hitherto been explored. But, in order to avoid controversy, it may be well to limit the range of my views to those bodies constituting the surface only.

373. Since, then, equal volumes of all the different kinds of matter, whilst surrounded by an equable electric pressure, are charged with different quantities of the electric matter, proportional to their susceptibilities of receiving it, it is obvious that those quantities would vary with every variation of the electric pressure, whether that variation of pressure were general on every side, or partial only, by its limitation to one particular portion of the surface. Let us, for instance, apply this reasoning to the well known experiment with Volta's plates of copper and zinc, than which, I know of no simpler case. See Fig. 1.

Fig. 1.



374. Prior to bringing the plates into contact with each other, each plate is surrounded by an equable electric pressure; and, consequently, each has its natural share of the electric fluid, due to its susceptibility of receiving it, under such pressure. But when the plates are brought into contact with each other, face to face, it is very obvious that the electric pressure, in the plane of contact, has received a material alteration: it is, in fact, *lessened* on the face of the copper, but increased on the face of the zinc: and a new distribution of the electric fluid takes place, in consequence of a momentary flow *from* the copper, through the plane of contact, *to* the zinc. The zinc having now received more of the fluid than it had whilst under the natural equable pressure, and the copper having lost that quantity, the former becomes positively, and the latter negatively electrical. The distribution, however, is now of a peculiar kind, and very different to the distribution on each individual body prior to contact: which, was equable on every side. The new distribution brings the pair of metals into an electro-polar state; the exterior surfaces of the zinc and copper being, respectively, positively and negatively electrical; not only with respect to each other, but with respect to the plane of contact, and also to exterior bodies

* *Annals of Electricity*, vol. ii, page 401.

whose natural circumambient electric pressure has not been disturbed-

375. If the metals be suddenly separated, whilst insulated, the redundant fluid on the zinc has not *time* to return to the copper: the former is, therefore, left in a positively, and the latter in a negatively electrical condition; but, if the separation be made slowly, the whole, or nearly the whole, of the redundant fluid will return to the copper plate: and the usual equilibriums on the two metals will be restored.

376. Now, since it is an invariable law in electricity, that no flow of the electric fluid can take place from one body to another, unless the former be positive to the latter,* the experiment with Volta's discs furnishes us with a piece of interesting information, which, independently of some such experiment, we had no means of arriving at. It shows us in the most decisive manner, that *prior* to contact, and whilst the metals were subjected to the *same* electric pressure, the copper was positive to the zinc; but that, after contact, the zinc was positive to the copper.† By proceeding cautiously, with similar experiments, on different bodies, the relative natural electric states of an extensive series of them might easily be obtained: this interesting piece of information, however, remains a desideratum, with the exception of a very few insulated facts.

377. If, whilst the copper and zinc plates were in contact, a copper wire were to connect their outer surfaces, a return of a small portion of the electric fluid, from the zinc to the copper, would take place, and a new distribution, and subsequent equilibrium, would be the results. But, as the transverse sectional area of the conducting arc, which rested on the surface of the zinc, would be extremely small, when compared to the whole area of that surface, the new distribution of the fluid, produced by the application of the arc, would be very little different to that displayed *prior* to such application: and would not be productive of any change in the *general character* of the polarization.

378. If, however, the conducting arc were of large transverse dimensions, or if a great number of small ones were employed, so as to cover, by their sectional areas, a large portion of the surface of the zinc plate, the previous distribution, due to the contact of the copper plate, would be very much altered. Nevertheless, the previously displayed polarization, although much lessened in *degree*, would still retain its original *character* on every portion of the exterior surface of the zinc that remained unoccupied by the ends of the conducting arcs; and it is not until the whole surface of the zinc is completely covered with copper, that the polarization ceases to be displayed.

I believe that this fact had never been noticed by any writer on electricity until I mentioned it in my *Familiar Instructions in the Theory and Practice of Electro-Gilding, &c.*, published in March last.

† I am well aware that many persons are of opinion that zinc is *invariably* positive to copper, but there are very few of them, if any, who have given a reason for entertaining that opinion.

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379. I have made extensive series of experiments on this delicate part of the subject, and from their results I have been led to conclude, that these facts are not limited to the employment of copper and zinc only, but that they are producible, and can be satisfactorily displayed by any two metals whatever: and not only by the employment of two *distinct kinds* of metal, but by two discs of one individual metal, cast from one and the same fused mass.

380. Now, when the polarization first takes place, by the simple contact of Volta's plates, and the new equilibrium has become established, it is obvious that the electric force on the zinc surface has become increased, and that on the copper surface diminished. Hence it is, that the respective electric tendencies of those surfaces, with reference to the circumambient medium, are very different: the zinc having a tendency to *dispose* of a portion of its fluid, and the copper a corresponding tendency to *receive* a portion. Under these circumstances we have an opportunity of either increasing or diminishing the sum total, or general stock, of the electric fluid in the two plates. If, for instance, I touch the copper plate with an uninsulated conductor, a new portion of fluid is transmitted to its previously negative surface, and the general stock is increased: but if, on the contrary, I touch the exterior surface of the zinc plate with a similarly situated conductor, a portion of the fluid, previously accumulated on that surface, is delivered over to the conductor, and the general stock becomes diminished. A moment's contact with the conductor, in each case, is sufficient to produce the effects stated: which may be proved by separating the plates slowly afterwards. In the former case both plates are found to be positively electrical; and in the latter case, both are found to be negatively electrical.*

381. The principles on which the display of the above described phenomena depend, may be conveniently taken advantage of in exhibiting the electric conditions of the plates by their simple contact: because the action of each individual plate is enhanced by applying a conductor to the other, whilst they are in contact. If, for instance, I wish to show the positively electric action of the zinc to the greatest advantage, I insulate that plate only, and not the copper: and, on the other hand, if I wish to show the negative electric action of the copper plate to advantage, I insulate it only, and keep my hand in contact with the zinc. By these means, a single contact is sufficient to show the electric character of each plate.

382. Hitherto I have noticed the electro-polarization of the zinc and copper plates, only as a group, or a collective whole; but it will be easily understood that, since the exterior surfaces of the zinc and copper are respectively positive and negative to the plane of contact, each *individual* plate is also electro-polar: the *outer* and *inner* sur-

* For the performance of these experiments the most delicate electroscope must be employed. That described at page 427 of this volume, is the one used in my investigations.

faces of the zinc, and the *inner* and outer surfaces of the copper, being positive and negative respectively.

383. Now, although the two surfaces of a zinc plate would not become, relatively, in positive and negative states from the contact of each with a plate of copper of similar dimensions, their electrical conditions would become changed, and the whole group would become electro-polar: both *collectively* and *individually*, as decidedly as in any other case. To show this fact satisfactorily, the zinc plate ought to be pretty thick, and insulated: and the copper plates held in the hands by metallic handles. When the contact is completed on both sides of the zinc, and the copper plates afterwards suddenly and simultaneously separated from it, it is invariably left in a positively electric state. The fact is also shown by reversing the experiment; and operating with insulated copper plates and uninsulated zinc. Let the zinc plate be supported on an uninsulating pillar, with its plane vertical: and let each of its surfaces be covered with an insulated copper plate. On separating the latter plates suddenly from the zinc they will be found negatively electrical.

384. Nothing could be more satisfactory in establishing a general law in electricity than the facts here stated: viz. that, *by the simple contact of dissimilar metallic bodies, a partial transfer of the electric fluid from one to the other invariably takes place.* This is not only a general law in electricity, but also one of the *fundamental laws* in *Voltaism*, or voltaic electricity, in all those associations in which two dissimilar metals are employed: and the same law is applicable in all other voltaic associations, whatever may be the character of the materials which enter into them.

385. Therefore the first step in every case of voltaic electric action, is a transfer of electric fluid from one of the bodies to another. This first move of the electric fluid gives rise to a new electro-distribution to electro-polarization, and, in those cases where the group is *insulated*, to a new electro-equilibrium.

386. Having thus satisfied ourselves respecting the primary and the secondary electric conditions of each individual pair of metals, our next consideration is to ascertain what takes place in a *series* of pairs placed within the sphere of each others action, the simplest of which is that of the dry pile.

387. When two pairs, A and B, are placed in such a manner, with respect to each other, that the positive surface of A be directly opposite the negative surface of B, having only a thin film of air between them, the previous electro-equilibrium of each pair will again be disturbed; for the accumulated fluid, on the *inner* positive surface of A, will charge the thin film of air, and thus cause it to exert a greater electric pressure on the vicinal *negative* surface of B than that to which it was previously exposed; and no corresponding pressure taking place on the exterior or positive surface of this latter pair, its fluid will be urged in that direction, and, consequently, the accumulation of fluid on that surface of B will be increased. The disturbance of the fluid in A arises from the electric

pressure on its inner or positive surface being diminished by the vicinal negative surface of B, without any corresponding diminution of pressure on its outer surface, the first effect of which is a movement of the fluid in A towards B. It therefore appears that the fluid, in both pairs, moves in one and the same direction, and that the ultimity is a new equilibrium in the group, in which a more powerful electro-polarity is established than can be displayed by either pair alone.

388. It will now appear very obvious, that, if a third pair C, were to be added to A and B, the electro-polarity at the extremities of the series would become still greater than that displayed by a group of two pairs only; and, for the same reason, every additional pair would cause an increase of polarity in the series which, when extended to about 100 pairs, would be sufficiently powerful to affect electroscopes, and put light pendulous bodies into motion.

389. The electro-polarization of bodies may be enhanced either by augmenting the disturbing force, or by lessening the resistance of the surrounding mediums. The latter circumstance is usually resorted to in the construction of the electric column, in which discs of dry paper, instead of films of air, form the intermediate medium between the metallic pairs.

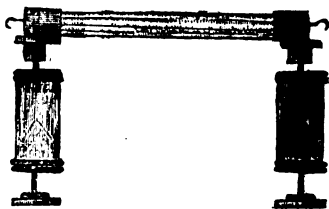
390. M. Marechaux was the first philosopher who employed paper in the dry electric column. The metals in M. de Luc's columns were discs of thin zinc, and of Dutch gilt paper; the gilt side of the paper being in contact with the zinc in every pair. The series was strung upon a silken thread, which passed through the centre of the whole, and then placed in a glass tube furnished with brass caps and hooks at its extremities, as represented by Fig. 2.

Fig. 2.



391. When one extremity of the pile is held in the hand, and the other to the cap of an electroscope, the gold leaves immediately diverge, indicating the electric character of the pole in contact with the instrument. Or the column may be placed horizontally on the caps of two gold leaf electroscopes, as represented by Fig. 3. In this case both instruments indicate electric action in the extremities of the column,

Fig. 3.



the one positive and the other negative. By a series of 20,000

pairs the late Mr. Singer was enabled to charge Leyden jars by a moment's contact. By the employment of coated talc, instead of glass, I find that an extensive surface may be charged by a dry pile of 10,000 pairs.*

392. The principal consideration, in this place, is the perpetuity of the action of the dry electric column; which, when properly constructed, and securely guarded against moisture, appears to be possessed of interminable electric powers. At the time when philosophers, especially the chemical part of them, could form no idea of the changes in the electric characters of bodies but such as emanated from *oxidation*, the most fruitful of all sources of error, in this branch of physics, the dry electric column shared the same fate as the wet pile of Volta, in having its action placed to the credit of oxidation. And even when it was found that the dry column would retain its action for several years, a *slow* oxidation of the metals was the only explanation that could be given for the display of its electricity. This idea became so fashionable eventually, that it became the basis of the prevailing hypothesis which, to this day, is strongly contended for by certain philosophers now in controversy on this interesting topic.

393. To those who have paid attention to the succession of steps that I have hitherto taken in tracing the electric action from metal to metal in each individual pair, and also that action which is due to a series of pairs, as associated in the dry electric column, there can appear no reason whatever for calling into the hypothesis an oxidation of the metals; but, on the contrary, since purity of the metallic surfaces is essential to the development of those peculiar arrangements of the electric forces which I have described, and which form the very soul of the dry electric column; and since the introduction of an oxidizing process would soon pollute these metallic surfaces, and ultimately destroy every particle of their metallic character, nothing can appear more evident in physical science, than that *a perpetuity of electric action in the dry pile, depends upon a continuance of the purity of the metal.*

394. The inference thus drawn from the simple principles of electricity, is that alone which can direct the *practical* electrician with certainty to the construction of a *permanently* acting electric column. It is conformable with all experience, and, I believe, is now acted upon by those who make the best apparatus of this kind. Every care is taken to insulate the elements of the column from the atmospheric air, and from every kind of moisture; which, when properly accomplished, the permanency of electric action becomes secured.

395. As the whole action of the column depends upon electric pressures, any change in the external electric pressure of the atmos-

* The whole process of forming *dry electric columns*, with a variety of their interesting applications, will be clearly described in my *Cyclopædia of Electricity and Magnetism*, shortly to be published.

phere will necessarily affect the action of the apparatus. It is on this account that it becomes an indicator of those fluctuations of atmospheric electrical pressure which are almost continually going on; and, as these fluctuations are occasioned by change of temperature, humidity, evaporation, winds, barometric pressures, clouds, &c., and frequently by several of these causes at the same time, there can be no astonishment excited at M. de Luc's failures in attempting to apply the electric column as a meteorological instrument. The frequent changes in the activity of this apparatus, first noticed by that ingenious and indefatigable philosopher, are exceedingly curious and interesting. He attached to the upper pole of a vertical electric column, a bent wire, which reached downwards as low as the lower pole, and terminated in a small brass ball. Between this ball and the lower pole of the column was suspended, by a silken fibre, a light gilt pith ball; the whole being covered by a glass shade, as represented in Fig. 4. A pendulous ball vibrates between the lower pole of the column and the opposite ball, carrying the electric fluid from one to the other continually. When the atmosphere is highly charged with the electric fluid the ball vibrates with great rapidity; it also moves rapidly in a warm room, or any warm unattenuated atmosphere, but languishes very materially in a moist atmosphere.

396. When two columns are arranged as in Fig. 5, having a bell attached to each lower pole, the one positive, and the other negative, the pendulous brass ball plays between them, and rings both bells; thus warning the observer when any material atmospheric electrical change is taking place. Apparatus of this kind have kept in play for upwards of twenty years, and are still as active as at first; and there can be no satisfactory reason shown why they should ever cease to display their electric forces.

397. Having now disposed of the views which I have taken of the theory of the dry electric column, I shall next endeavour to explain the principles upon which the wet pile, or voltaic battery, operates; and why its action soon languishes, and ultimately becomes annihilated. And in this attempt, as in that of the previous part of the subject, I shall proceed with the simplest case.

398. The simplest case in the action of the wet pile of Volta, is similar to that of the dry pile, already discussed; and the action of such a pile, under certain circumstances, would be as durable as that of the pile of M. de Luc, or of any dry pile whatever, provided the

Fig. 4.

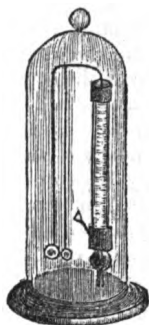
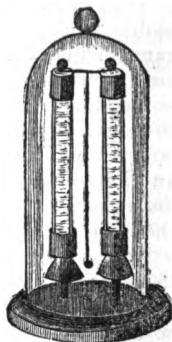


Fig. 5.



electric condition of each individual metallic surface were uniform ; but, in consequence of an ununiformity of electric action on various points of every metallic surface, as already explained in the first section of this memoir, any moisture, in contact with such surface, would suffer partial decomposition, and the metal itself undergo a change, whether employed as an element in a voltaic series or not.

399. When, however, pure water is employed for the intervening medium between the metallic pairs, the pile assumes electro-polarity in a supereminent degree : and a series of about five hundred pairs would charge an extensive coated surface of glass, by one momentary contact of either of its poles. Five hundred square feet of coated glass have been charged, by a pile of this description, to a higher degree of intensity than can possibly be accomplished, in the same time, by the most powerful electric machine : and by increasing the extent of the voltaic series, the charge would increase in proportion. Hence, from a source of this kind, electricity, to almost any required extent, might be obtained. And, although the local electric action, especially on the surface of the zinc, tends continually to lessen the general electric forces of the pile, and eventually annihilates them altogether, the apparatus retains a considerable degree of action for many successive hours. The theory of the general action of this apparatus is precisely that already given for the dry electric column ; and it is a matter of no consequence, whether the metals be piled one upon another, with intervening moistened paper, as represented by Fig. 6, or that they be placed edge-wise, in a Cruikshank's trough, represented by Fig. 7, whose cells are

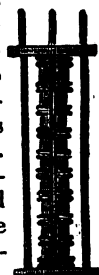


Fig. 6.

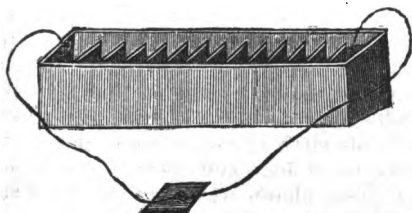


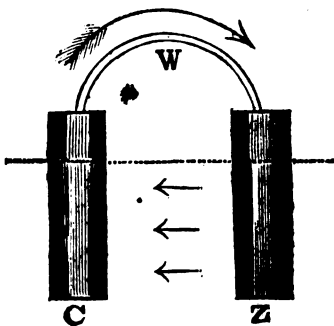
Fig. 7.

newly filled with pure water. In this latter form, however, the cells must be perfectly water-tight, and the pairs of metal well insulated from each other by resinous cement.

400. When the voltaic pile, or battery, is employed in its most usual capacity, for the production of electric currents, the principles hitherto described form a part only of the theory of its action ; for *stable* electro-equilibrium is inconsistent with the idea of a continuous flowing current ; therefore, other principles must enter the framework of the theory, in order that its explanations may become satisfactorily applicable to every variety of case.

401. When a pair of copper and zinc plates are united by wire, as in Fig. 8, the pair becomes electro-polar as decidedly as in any other case; but not so powerfully so as when the planes of the plates are laid parallel to each other, as in Fig. 1. They, however, still form a voltaic pair, and the zinc receives a portion of the electric fluid, previously belonging to the copper, and consequently, is in a suitable condition to give up that portion to any conducting body capable of receiving it. The copper plate, also, being now in a negative state, is equally prepared to receive a new portion of fluid from any body appropriately situated to communicate it.

Fig. 8.



402. Let us now suppose that the metals are immersed in water, which is a compound body, and whose particles are susceptible of motion by the application of slight forces, and of separation from one another by the introduction of other matter amongst them: also that the electro-conduction of water is improvable by admixture with acid, and other liquids; and its constituents held together by certain electric forces, and consequently susceptible of separation by superior electric forces. Under these circumstances, the first immersion of the plates into pure water, would cause a movement of their electric fluid, in such a manner, that the zinc would give up a portion to the water, and the copper would receive a portion from it: and if nothing further went on, there would be a new distribution of the electric fluid, and again a statical polar equilibrium established, as decidedly as under any other circumstances.

403. With copper, zinc, and water, however, the electric forces of the metals are somewhat more powerful than those which unite the oxygen and hydrogen in the shape of water: and as the particles of water themselves are electro-polar, those of them next to the plates become easily arranged in regular polar order, with respect to the electric forces of those plates. The positive zinc surface attracts, and draws towards it the negative surfaces of the particles of water: and the positive surfaces of another stratum of water become placed in juxtaposition with the negative surface of the copper. The electric forces of both the metals and the water being now arranged in the best possible manner to accomplish a separation of the constituents of the latter, and subsequently urge them in opposite directions, accordingly to their relative electric characters, the combined forces, thus arrayed, vanquish those by which the constituents of the water were held together, conveying the hydrogen to the copper and the oxygen to the zinc.

404. Now, since we are unable to discover, by observation, at what

part of the water its decomposition takes place, the theory is necessarily left in some degree of obscurity on this particular point, which has given rise to much inconclusive discussion, and to opinions of many diverse kinds. In philosophical reasoning, analogy, in the absence of phenomena, often becomes a valuable substitute; and data, thus supplied, have led to inferences as satisfactory as if drawn from facts themselves: in the present case, however, analogy seems to be productive of various conclusions, nearly all of which are equally supported. It is known, for instance, that alkaline and acid matter, though placed in separate vessels, are made to traverse a voltaic circuit, exterior to the battery, in opposite directions, even through each other,—to exchange places, and occupy each others positions in the two vessels. If this fact were to be made the basis of analogical reasoning, it would equally support either of two opinions, in the decomposition of water by voltaic electrical agency of a single pair, as in Fig. 8. It would be as applicable to the supposition of the decomposition taking place at the centre of the mass of water, between the copper and zinc, as to the supposition that decomposition occurs at both plates at the same time, transporting the constituents in opposite directions, in both cases.

405. It has long appeared to me, that, since all the metals, and carbon, which are the best conductors of electricity known, are invariably carried to the negative pole of the battery, or in the direction of the current, through the liquid part of the circuit, there is something like a general tendency for the electric fluid to take possession of the best conductors in the liquid mass, and carry them to the next solid conductor; as, for instance, to the copper, in a single pair. Should this be the case, it would be an easy matter to explain the reason of alkaline matter being invariably determined at the negative metal: for the potassium, or other alkaline metallic base, would arrive there as a pure metal; but being reoxidized as fast as liberated, it would reassume the alkaline state, and dissolve in the liquid. The same reasoning also applies to ammonia, strontia, and other compound bodies, which are known to arrive at the negative metal when, in connexion with other matter, they are submitted to the action of an electric current.

406. It is now some years since I attempted to show the correctness of this view (405) in the *Phil. Mag.*;* and I have not yet met with any fact that has tended to militate against it. I there showed that when a mixed solution of two metallic salts, the sulphate of copper and sulphate of zinc, is subjected to the action of an electric current, the copper, which is a better conductor than zinc, is carried *alone* to the negative polar terminal: and that by this means, a considerable portion of copper can be separated from the zinc. From this fact I had every reason to suppose, that, if any of the metals be compound bodies, as some have thought them to be, their decompo-

* I cannot lay my hand on the volume at present; but will take occasion to give the precise reference in another place.

sition might be effected according to the same law, and by a similar mode of operation. This hint was thrown out in the same paper. I have, however, found, that the results of all experiments of this kind, are modified by the strength of the metallic solutions. But the fact that *all the metals* (which constitute about one-half of the known simple bodies) being determined at the negative polar terminal, when liberated from their combinations with other bodies by electric currents, is strongly in favour of the views I have taken on this topic.

407. The term *electric current* has become so exceedingly familiar and convenient, that it is employed without hesitancy by almost every writer on the subject, though the universal taciturnity on the mode of propagation, leaves a sad blank in every hypothesis hitherto made public.* The principles I have already embodied in this memoir, although perfectly explanatory of the preliminary conditions essential to the production of a voltaic electric current, as far as electro-distribution and polarization are concerned, would leave the associated group of metals and water in a perfect electrostatical repose. But it has been shown (402) that such an equilibrium is not stable with such an arrangement of these materials, and that the first consequence is a decomposition of a portion of the water, which, once accomplished, motion is again produced in the electric fluid; and, so long as any part of the water is suffering a change from its constituents being torn asunder by the assailing electric forces, no stable equilibrium can be maintained, and the electric fluid, as a matter of course, is kept in a continual state of commotion. Hence it is, that, although the *decomposition itself* is the primary effect of the combined electric forces of the metals and the water, it is not only the *first step* in the production of a current, but absolutely essential to its existence and propagation.

408. Volta, whose theory of the pile was similar to that I have explained, with the exception of the view I have taken respecting the propagation of the current (407), considered that the water was a mere conductor between the metals; but it is easily demonstrated that *no electric current, nor electro-circulation, can possibly be produced independently of motion in some of the other elements employed in the arrangement*. I have shown, some years ago, in the *Phil. Mag.*, and I think in the *Annals* also, that, in the production of *thermo-electric* currents, where the *calorific* matter is the *motive-agent*, and in the production of *magnetic-electric* currents, where the *magnetic* matter is the *motive agent*, a disturbance of those agents is an essential preliminary to the *existence* of these currents, and an indispensable circumstance in their propagation. And it is equally demonstrable that, with whatever degree of force the electric fluid might be disposed to expand, and move in any one direction more than

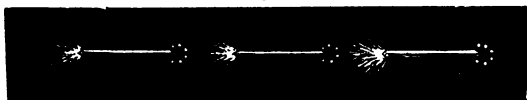
* In a small work on *Electro-gilding*, &c, lately published, and occasionally in other places, I have attempted to show by what means voltaic electric currents are propagated.

another, it would speedily equilibrate amongst such bodies as would not yield to its powers, and again assume a determinate statical repose. Hence, under such circumstances, no propagation of a current could possibly take place.

409. By some writers on this subject, the existence, even of a current has been questioned; but these are few in number, and those few have advanced nothing to the contrary but mere sceptical surmise; probably, more with a view of appearing singular, than from any train of reasoning they have bestowed on the subject. That a current does exist no experienced man can doubt; but, unfortunately for their readers, few writers are experienced men. We have some beautiful analogies in mechanical and voltaic electricity, and some peculiar phenomena exhibited in the latter branch, from whose direct inferences there seems to be no appeal.

410. If a series of pointed metallic wires were to be arranged, as in Fig. 9, and one of the extreme points placed near to the conductor of

Fig. 9.



an electric machine in good action, a luminous star would tip every point in the series that is directed towards the prime conductor; and those points of the wires which are presented in the opposite direction, or *from* the conductor, would exhibit a beautiful brush of electrical light. Now, since it is admitted that the star and brush are, respectively, indicative of a *receiving* and a *delivering* extremity in each wire, it follows, that an electric current is passing through the whole series, and also through the interposed plates of air. Moreover, it must be borne in mind, that a *delivering* surface must be positive to that which *receives* the electric fluid from it. Hence, we are led to understand that each wire and each plate of air is electro-polar, and that this polarity is essential to the existence of the current; and *vice versa*, the current is a consequence of the polarized state of the wires and interposed plates of air.

411. The counterpart of the last described phenomena, is beautifully displayed in voltaic electricity. Let a series of metallic wires be arranged in a glass tube holding acidulated water, as represented in Fig. 10,

Fig. 10.



and the two outer ends of the extreme wires in connection with the two poles of a voltaic battery. Every wire in the series becomes electro-polar, liberating hydrogen at those extremities which point towards the *positive* pole, and oxygen at those which point in the opposite direction. If the tube contain a blue neutral solution of

acid and alkaline matter, and the wires be platinum or gold, the well known characteristic red and green colours will shortly appear. Now since there is an electric current produced in the former case, (410) the analogy is sufficiently striking and complete to infer that a current is also in existence in the latter: indeed no other satisfactory conclusion can be arrived at, and since a series of wires similarly arranged *within* the liquid in a battery, display phenomena as decidedly as a series *without* it, in the glass tube, for instance, there can be no reason to doubt the existence of a current throughout the whole system.

412. The direct effects of an electric current are strikingly manifested by the different appearances of charcoal points, after deflagration; the *delivering* piece being pointed, and the *receiving* piece being rendered concave.

413. It is well known that I have, for many years, advocated the *non-identity* of the electric fluid, and the calorific fluid (First Memoir, 28);* and I know of no fact that supports these views more completely than that which I discovered in the autumn of 1838, with a battery of one hundred and sixty pairs of copper and zinc.† In that remarkable phenomenon, I can see no other mode of explanation than that which I have already given in my letter to Professor Silliman:‡ viz., that the electric fluid, which is an exceedingly *active* agent, was enabled by its volant powers, to spring from wire to wire, through the intermediate stratum of air; whilst the comparatively sluggish calorific fluid, being unable to traverse the aerial space with the same degree of velocity, was thrust *out* of the electric path, and forcibly compressed into the remotest extremity of the *positive* wire, and entirely exterior to the electric circuit.

414. But the same phenomenon, which is so happily applicable to the support of those views (413), is also demonstrative of the existence of a current; and perfectly conclusive of the direction in which it flows through the conducting wires, for reasons already given.‖ Moreover, the direction of the current in a voltaic circuit, is demonstrable by its magnetic effects, from the analogies which they exhibit with those manifested by the discharge of a Leyden jar. Since, therefore, the existence of electric currents in a voltaic circuit, are so strikingly manifested by the display of so many indubitable facts, those currents necessarily emanate from a primary cause; which, as I have already stated, (408), is not to be found in any group of unyielding and unalterable bodies; and, as the metals suffer no alteration prior to the decomposition of the water, this latter is the primary effect of the electro-polarization, and is essential to the production and propagation of the current.

(To be continued.)

* *Annals of Electricity, &c.*, vol. ii, p. 416.

† The particulars of this battery, and of the experiments in question, will be found in the *Annals of Electricity*. vol. v, p. 365: also in Silliman's *American Journal*. In one of my lectures, at this Institution, in the present month, May, I produced the same phenomenon by fifty pairs of Groves' battery.

‡ *Ibid.* ‖ *Ibid.*

Experiments on Bichlorure of Sulphur and certain Carbures of Hydrogen, made in the laboratory of Jefferson College (Louisiana). By Prof. F. CHEVET.*

A CURRENT of bicarbure of hydrogen being brought to bear on some bichlorure of sulphur, under the influence of the solar rays, the gas was absorbed in considerable quantities with a great throwing off of heat. The liquid, at first of a very deep pomegranate red, gradually became orange coloured, then of a yellow orange colour. The light refracted by the ball containing the bichlorure, assumed the finest violet hue, like that produced by vapour of iodine. This colour lasted a very long time.

Among the vapours thrown off at first, the writer thought he remarked chlorohydric ether and sulphohydric ether. These vapours made the water through which they passed milky, but this effect soon ceased. Towards the end of the operation, the gases evolved burned with a very fuliginous flame, like bicarbure of hydrogen, pure; however they by no means had the same smell. There appearing to be no absorption, the liquid was gradually heated to bring about absorption, by producing an atmosphere of vapours; suddenly the yellow liquid assumed a raspberry red colour, but no marked absorption was effected. Its bulk was very viscous, like a thick syrup; its smell was penetrating and very enduring, similar to that of blackberries or raspberries; its flavour was at first sweet, then very pungent.

The next day, a deposit was found of a number of small needle-shaped crystals of a deep brown.

Neither water, alcohol, nor ether, appears to dissolve these crystals to any decided amount; however alcohol discolours them, whilst it colours itself and leaves a drop of red liquid by evaporation. Water casts off from the alcoholic solution a white powdery deposit, and leaves a red drop at the bottom of the vessel. Nitric acid, cold, appears not to act, but warm, it dissolves the crystals and gives a yellow sediment of sulphur.

The crystals, having been several times washed with alcohol, assumed a light chocolate colour; after being strained through pieces of blotting paper they were discoloured, leaving on the paper a very volatile oil which rapidly disappeared, but there remained a red stain on the paper, which shows that the volatile oil is distinct from that species of colouring matter. The crystals strained through paper, were placed in the pneumatic vacuum in the presence of sulphuric acid, and moist fragments of potash; the surface of the acid became of a decidedly roseate hue, and besprinkled with small oily drops; the potash had absorbed some of the chlorine. These crystals were then pretty white, and burned in a very lively manner, bubbling up and emitting a flame which betokened the presence both of sulphur and of a resinous matter. Sulphuric acid appears

* Silliman's *American Journal of Science*.

powerless on these crystals, unless it be that it discolours them. Another part of the crystals having undergone a potash solution, gave a solid and very gluey deposit of a dirty yellow; the solution became yellowish; the sides of the vessel in which the operation was performed, became very greasy; the deposits, as well as the potash solution, had a very strong and decided smell of cucumber.

The red liquor, in the midst of which was the mass of crystals, slightly smoked in the open air, though its point of ebullition was pretty high; its density is greater than that of sulphuric acid, but a part floats even above the water, which betokens a complex liquid; it is insoluble in water and in ether, but rather soluble in alcohol; however, the alcoholic solution having evaporated, appears to leave the liquor untouched; water brings about a powdery deposit of a currant red.

This liquid, on distillation, gives a yellowish oil of a flavour acrid, pungent, and very enduring; it reddens the blue paper, doubtless by free chlorohydric acid.

The writer further made bichlorure of sulphur, prepared cold, react on two other carbures of hydrogen, oil of naphtha, and essence of turpentine, both as highly rectified as possible. With the oil of naphtha, the action is lively, and accompanied by a marked ebullition; the temperature rises rapidly, and a considerable quantity of chlorohydric acid is thrown off. A black deposit of a very glutinous nature was obtained; the liquid assumed a very brown red colour. The whole, being distilled, gave a yellowish liquid, which, being washed with water, furnished a yellowish and glutinous mass, floating on the liquid; it was sulphur impregnated with a very volatile oil, rapidly disappearing from the paper used for straining, and without any sediment. This mass undergoing a warm preparation with alcohol, considerably diminished in bulk, and, after cooling, oily drops gathered on the surface. Ether dissolves this species of oil better than alcohol; what remained undissolved by the ether, still betokened, on being burned, the presence of a resinous matter; it was then subjected to the influence of boiling nitric acid, which left a globule of sulphur. The washings of the distilled liquid contained much chlorohydric acid, and also some sulphuric acid.

The deposit left in the cucurbite became blacker and more plentiful; it burned like resin, and did not appear to contain sulphur; it is soluble in nitric acid, warm, and after a suitable evaporation, it deposits very long needle-shaped crystals of perfect whiteness; these crystals are of a slightly bitter flavour, and have no feature of acidity.

With the essence of turpentine, the reaction is extremely tumultuous. The vessel in which the operation was made was sunk in cold water, and yet the matter boiled up considerably; the mass became very viscous, but it remained homogeneous. Distillation was performed; a great quantity of chlorohydric vapours were thrown off by the draught tube; a pomegranate-red liquid condensed in the recipient; this liquid exhaled a stinking smell, pretty similar to that

from the products of the distillation of animal matter. A very black sediment remained at the bottom of the cucurbit. On applying nitric acid to this mass, no needle-formed crystals were obtained as with the naphtha; the washings of the distilled liquid gave a very viscous reddish mass, which sank to the bottom, instead of the floating yellowish mass obtained with the naphtha.

Not having at my disposal the means and appliances for proceeding to organic analyses, I have been unable to ascertain the composition of the different products to which the reactions above described gave rise: a composition, the knowledge of which is indispensable to a correct appreciation of these products. In publishing this memorandum I have therefore had no other object in view but to point out a few facts relating to the action of bichlorure of sulphur on carbures of hydrogen—facts which have appeared to me worthy to engage the attention of chemists, and susceptible of being connected with one another, and brought under the laws of a common theory.

Deprived of the apparatus and reactives necessary for this study, I have deemed it my duty to give publicity to an entirely novel subject of inquiry, which, in my opinion, holds out a certainty of important discoveries; and I hope and trust that some American chemist, placed in circumstances more favourable, and especially one more skilful, will, by following up this subject of inquiry, ere long enrich the science with several new and interesting combinations.

MISCELLANEOUS.

Cast Iron Battery.

IN answer to our Arnheim* correspondent's enquiries in a letter, dated March 3rd, respecting the charge of acidulated water for the cast-iron battery, described in vols. v, and vi, of the *Annals*, we would say, that no charge of acid liquor will keep it in *constant* action for two successive hours. The proportions of sulphuric acid to the water, for the best action, would be as 1 to 6, or to 5, but the action suffers a diminution in half an hour, though the battery will keep in good action for more than two hours. With a less proportion of acid, the action is more equable for a longer period; and an occasional renewal of acid liquor will keep it in good action for any required time; this is also the case with all batteries: they must be occasionally fed to keep up their actions.

With respect to the electro-gasometer, we will describe that which we employ in our next number; but we would remark in this place, that the platinum plates for twenty-four pairs of metals, in a double series of twelve, which is the best arrangement, each plate of the decomposing apparatus should not be less than eight inches square: thirty pairs is not a good number for decomposition of acidulated water.—EDIT.

* In Holland.

CALOTYPE PAPER.

To the Editor of the Annals of Electricity, &c.

SIR,—In my note on the preparation of calotype paper, in your last number, page 144, there is a slight mistake, which, as it may have arisen from my mistake in writing, I hasten to correct. It is this: “nitrate of silver to contain 1·20 grs;” for which read “nitrate of silver to contain 120 grs.” The strength of this solution may, however, be considerably varied, as the proportion of the salt used by Mr. Talbot answers very well for many purposes.

I am, Sir, your very obedient servant,

C. J. T.

ELECTRO-MAGNETISM AS A MOVING POWER.—The Consul-General of the Netherlands, in a late communication, announces the removal of the obstacle to the practical application of electro-magnetism as a propelling power:—“A private gentleman, Mr. Elias, of Haarlem, has just published the description of a new machine invented by him, for the application of electro-magnetism as a substitute for steam. The object of the inventor has been chiefly to remedy the defects which, in 1839, rendered the otherwise ingenious invention of Mr. Jacobi, of St. Petersburg, a total failure, in as far as practical utility is concerned. Those defects originated, it seems, in the erroneous supposition, that the power of the magnetic bars exclusively resides in their extremities—whence the form hitherto given to all electro-magnetic machines; viz., that of a horse-shoe,—which while it occasions an unavoidable interruption of the magnetic stream at each new inversion of the poles, at the same time leaves the power resident in the remaining part of the bars wholly unemployed. *The new invention of Mr. Elias, on the contrary, has the very great advantage of rendering effective the full power of the magnetic stream uninterruptedly, and throughout the whole body of the apparatus. This consists of two concentric rings of soft iron, standing on the same plane, of which the external one is immovable, whilst that on the inside revolves round its own axis. By means of a piece of copper wire, wound about each of these rings, he has given them six magnetic poles, placed at equal distances from one another; the whole being so contrived that the one ring exerts its inducing power on the other throughout the whole circumference, and always at the same distance. A small, but very perfect, model of this important invention is now open to public inspection here; and the result of its operation is allowed, by those skilled in such matters, to be such as to ensure the most triumphant success.”—*Mining Journal*.

* For an illustration of this force see “Sixth Memoir,” *Annals*, vol. vi. p. 295.—*EDIT.*

PHOTOGRAPHY.*

M. ARAGO wished to be heard, in order that he might make known some essential rectifications to the verbal communication which he had made on the preceding Monday, in reference to M. Daguerre. The inaccuracies committed by the secretary have been in consequence of explanations very little developed which had been given him, on the subject of new photographic processes. It is even to be feared, that the letter of M. Daguerre will not entirely satisfy philosophers, after the lively interest and curiosity which the results announced on Monday last would naturally excite. We will, however hasten to make them known : the facts remain the same as they previously were ; the corrections of to-day do not remove any thing which they had appeared to offer of the marvellous, it being of no matter, in effect, whether the instantaneous action produced by the interposition of electricity is produced on this or that substance. M. Arago again adds, in order to excuse himself to those experimenters who since the announcement of Monday have vainly endeavoured to excite by the electrical spark the ioduret of silver ; that neither he nor M. de Humboldt heard the word *acidulée*, when the ingenious artist imparted his new discoveries to them. We add a copy of M. Daguerre's letter to M. Arago :—

“ My dear Sir,—I have this instant received the *Compte Rendu* of the session of last Monday, and I perceive that an error has crept into the communication which you have there made of my new method.

“ It is in speaking of the *iodized* plate. Though I do not think that the ioduret of silver is insensible to the electric fluid combined with the action of light, I have not as yet found any magnet, either direct or indirect, capable of forming the image with the vapour of mercury ; for otherwise the process will be now as complete as the first, since the face of the plate so prepared is now so sensitive as not to allow time to open and shut the diaphragm before and after the electrical discharge.

“ You will doubtless recollect that I spoke to you of a plate *acidulée*.

“ As many persons have, without doubt, already tried to experiment upon my new method, by making use of a plate *iodurée*, submitted without any intermedial to the vapour of mercury, they will necessarily not have been able to obtain any thing. I trust, then, that you will be willing, as soon as possible, to destroy the word *iodurée*, which has led them into error. Besides, this communication ought not to be considered but as giving solely the invariable principle of the method.

“ I feel so much the necessity of occupying myself in the most active manner possible on these experiments, that I quit all my works in order to give myself entirely up to them.

• *Compte Rendu*.

"I thank you a thousand times, my dear Sir, for all the trouble that I give you, and I pray you to accept my lively gratitude.

"DAGUERRE."

"Bry-sar-Maine, 5th July, 1841."

THE ARTESIAN WELL OF GRENOBLE.*

At Grenoble, in the vicinity of the French capital, it was considered advisable some years ago to endeavour to procure good water by means of an Artesian well. M. Mulot d'Épinay was the engineer to whom the task was entrusted. On the 31st of December, 1836, the bore had been carried, after immense labour, to the depth of 383 metres (a metre is 3 feet and 2-10ths English). The soil was a clay, very hard and compact. In the month of June, 1839, the bore had reached the depth of 466 metres, and the soil was still a bed of clay, though a variety of strata had been previously passed. M. Mulot kept a regular journal of observations, relative to the soils and strata penetrated, and the temperature at different depths. This record will be valuable when published. At length, after a task of seven years one month and twenty-six days' duration, M. Mulot was rewarded by a degree of success proportioned to the time and trouble expended. Water was not only found, but found under such circumstances, and in such quantities, as will cause the well to be one of the most useful works, as well as one of the greatest marvels of artistical ingenuity in France. The fluid burst out in a perfect torrent, rising to the surface of the bore to the amount of nearly three cubic metres in a minute, or 180 metres in an hour, and 4320 metres in the twenty-four hours. Such is the force with which it flows up the shaft, that it mounts more than thirty-two English feet above the surface of the ground. M. Hemery, director of roads and bridges, has calculated that the force of ascension of the water, at the bottom of the shaft, exceeds, by fifty times, the force with which water rises in a vacuated tube of thirty-three feet. The orifice of the well is fifty-five centimetres (about one foot eight inches) in diameter, and at the bottom it is eighteen centimetres in diameter. The shaft is in all 547 metres (or 1630 French feet) in depth, and the sides are strongly plated with iron to a depth of 539 metres. The dome of the Invalids, which has an elevation of 300 feet above the ground, is thus only about a fifth of the perpendicular measurement of the Artesian well of Grenoble.

Three times, during the operations, did the shaft give way, but the indefatigable engineer was not daunted, and at last he has had his reward. The water, which the well pours forth incessantly, has converted one of the neighbouring streets into a river, but the workmen are at present employed in forming a channel for its proper con-

* *Mining Journal.*

veyance from the spot. As might be expected, the fluid was at first mixed with sand and earth, and continued to be so for some time. It is perfectly sweet, however, and had no odour of a disagreeable kind, or any other deteriorating qualities. It is of such a temperature that there is an obvious smoke arising from it when it reaches the surface. This is a feature not likely to continue, and indeed easily removable before use. The whole cost of this great work of art to the city of Paris is said to have been 160,000 francs. The perseverance in this labour for such a period of deferred success, is to be ascribed to the confidence resulting from modern geological discoveries; and the value of these is most splendidly shown by the success attained. By an ingenious contrivance, M. Mulot has been able to raise large quantities of sand from the bottom of the well; thus clearing the water more rapidly, and also adding very considerably to its force and volume. This removal of the sand has been attended with curious consequences in more respects than one. After ceasing, in a great measure, to throw up sand, the well has begun to throw up shells and petrifications of various kinds, the debris of a former world. The success of the operations of Grenoble has also induced engineers to make similar attempts in other quarters. One is begun on a large scale at Vienna.

WHIRLWIND IN LIVERPOOL.—On Thursday, May 11, at half-past three o'clock in the afternoon, the inhabitants of Bond-street and the lower part of Burlington-street, were thrown into a complete state of excitement and amusement by the occurrence of a whirlwind. The empty space of ground between the two streets is a hollow, of about twenty feet in depth, and is much used by the inhabitants for drying clothes, for which purpose posts are fixed in the ground at convenient distances, to which lines are attached. At the time above-mentioned, the hollow was completely filled with clothes, drying upon the lines; for some time previous there was not a breath of air, when all of a sudden it began to blow very hard, and what appeared strange was, one part of the clothes streaming out in one direction, and another part the contrary way. In a short time the currents met, and the lines were almost instantly stripped; some articles of clothing were carried in a straight line upwards to an altitude of about 500 feet, when they seemed to have got into a different current from that of the smoke of the neighbouring chimneys, the latter blowing southward, while the clothes were carried away in a northerly direction, and they have never since been heard of, at least, by the owner.

ELEMENTARY LECTURES ON ELECTRICITY, &c.

LECTURE XVII.

IN persuing our illustrations on the electro-mechanical agency in the production of certain effects, it may not be amiss to employ different kinds of bodies in the circuit, in order to show how they are differently affected by modifying the mechanical action in the discharges of similar quantities of the electric fluid upon them.

I will, for instance, place a small loose ball of tow between the balls of the universal discharger, and discharge upon it a large Leyden jar, from a high degree of intensity. You observe that the tow, although an inflammable body, is not ignited; but several of its fibres are broken, and it is somewhat flattened by the mechanical action of the discharge.

Now, although the tow has escaped conflagration in this case, it does not always remain unignited by operating upon it in this manner. Its ignition, however, is very much facilitated by scattering some finely powdered rosin amongst its fibres, as you will presently see, by sending a discharge of a similar quantity of electric fluid through it.

Now, in order to convince you that the ignition of the tow and rosin is a general result of this mode of operating, I will repeat the experiment a few times, and you will then have ocular demonstration of the fact.

If, instead of rosin, I sprinkle the tow with oil of turpentine so as make it somewhat moist, you will see by a few experiments that the ignition is as easily and as regularly accomplished as by the employment of the rosin. We occasionally employ other inflammable matter amongst the fibres of the tow, for purposes of this kind; and when tow is not at hand, other fibrous matter, such as cotton wool, is resorted to, but sheeps' wool is seldom, if ever, employed.

In all these experiments you will have observed, that a considerable report attended each discharge of the jar, which, as I have before shown you, can take place under no other circumstance than when the velocity of the electric fluid is very great; for if we abate the velocity, by passing the fluid through an inferior conductor, the wet string, for instance, scarcely an audible noise attends the discharge. But for fear of taking too much upon credit, I will vary the experiment in different ways, and I think the results in every case will serve as so many facts, in the direct process of demonstration.

In the first variation of the experiment I will again place a ball of tow, sprinkled with oil of turpentine, between the balls of the universal discharger; and in another part of the circuit I will place another ball of tow, sprinkled with powdered rosin, between two other insulated balls. Between the inside of the jar and the first portion of tow, I place in the circuit a piece of tinfoil, between a few leaves of writing paper; and on the other side of the latter ball of

tow I place some loose gunpowder. Now, by this arrangement, the electric fluid will have to traverse the whole of these bodies; first, through the paper and tinfoil; next through the tow and turpentine; then through the tow and rosin, and after leaving that combination of inflammables, it will traverse the loose gunpowder, and proceed to the outside of the jar. The results of the discharge of the large jar, from a high intensity, are easily predicted by those conversant in experiments of this kind; but to others there may appear to be something mysterious in them. Now we will discharge the jar. The result on the two balls of tow are probably such as you might be led to expect—both are set on fire; but what has become of the gunpowder? It also is an inflammable body, but it is not ignited, nor, indeed, is it left in its place. It is blown away, and scattered in every direction, from the axis of the electric path; not even a single grain is left where the fluid passed through the heap.

You will probably ask how I know the path of the discharge through the gunpowder, now that it is gone from the place? This is very easily ascertained; for if you will examine the card on which the powder was placed, you will find it perforated by the electric fluid, and by calling to your recollection the position of the gunpowder on the card previous to the discharge, you will identify the perforation with the centre of the base of the heap. Consequently the electric fluid, which has so obviously pierced the card, must have traversed the gunpowder *from* the pointed wire above it, *to* the perforation of the card.

But you will observe also that this perforation is the *centre* of the blank space on the card left by the gunpowder. Hence we have, by this fact, a complete demonstration not only of the existence of the lateral force of the discharge, but also a demonstration of that force being exerted on every side alike.

Now call to your recollection that you have already seen that no card can be pierced by a discharge of this jar when a wet string forms a part of the circuit; and that I have shown you that, through such a circuit the velocity is much lessened. From a knowledge of these facts you are led to understand that the velocity, and consequently the mechanical action of this discharge, must have been great, otherwise the card could not have been perforated. Moreover, the scattering of the gunpowder is another manifestation of a great velocity and consequent momentum of the discharged electric fluid.

Now let us examine the paper and tin foil in the other part of the circuit. Both are perforated: another indication of the great electro-momentum attending the discharge of the jar through this compound circuit.

Let us now make a slight change in this experiment, by introducing a piece of wet thread as a portion of the circuit. The jar shall be charged to the same intensity as before, but the result of the discharge will be very different. Neither the paper nor the tin foil will be perforated; neither of the balls of tow will be burnt, but you will see the gunpowder exploded. If, however, I were to enlarge

the lateral dimensions of the aqueous part of the circuit so as to increase the velocity of the fluid, and thus permit it to move in a more compact body, the paper and tin foil would suffer perforation, and the whole of the inflammables would take fire.

It is a remarkable fact that the gunpowder should not ignite under the same circumstances that accomplish the ignition of the other bodies; but *time* seems to be an essential element to insure success, and it is a matter of no consequence what are the means employed to give *sufficient time* for the electric fluid to be in contact with the gunpowder, for it will invariably ignite provided a sufficient period of time be procured, and the quantity of electric fluid transmitted be sufficient also.

Gunpowder has been ignited by a series of heavy sparks from a large prime conductor transmitted through a compact cartridge; but, in this case, the quantity of fluid was great, and the velocity lessened by the retarding power of the gunpowder itself, which being confined could not be blown away. Heavy discharges, with great velocities, from a battery of jars, have also been the means of igniting gunpowder; but in those cases the ignition of the powder was a secondary effect, arising from the wire which led the fluid into the cartridge being intensely heated by the discharge of the electric battery. Formerly it was a common practice to mix the gunpowder, intended to be exploded by electricity, with iron filings, which, becoming red hot by the discharge, set fire to the gunpowder as a matter of course. And we are not without those, at the present day, who conceal a thin platinum wire in the powder, for the purpose of accomplishing the explosion.

Another method of illustrating the fact that the ignition of loose gunpowder requires *time* for the electric fluid to be amongst its particles, and a consequent abatement of the velocity with which it moves through a circuit of good conductors, is that of first placing a portion in a circuit of that kind; transmit through it a discharge of a certain degree of intensity of the jar, and you will find the powder blown away as in the compound circuit of paper, tin foil, tow, and gunpowder, when all the rest of the circuit was metal. Having satisfied yourself on that point, next vary the arrangement, by placing a wet thread in the circuit. The discharge of the jar from the same degree of intensity, explodes the powder.

I have already shown you that, by an introduction of the moistened thread as a part of the circuit, the electro-momentum is sufficiently abated to be deprived of the power of communicating a shock to a person through which the fluid passes. Now, from that fact, and from the fact also of accomplishing the ignition of gunpowder when the electro-momentum is thus abated, there can be no reason why the gunpowder should not be ignited by the *same* discharge of the jar, in the circuit of which a person is placed without experiencing the shock.

Now, in order to give a striking illustration of this fact, I will place a battery of six miniature pieces of ordnance in one part of

the circuit, and myself in another part of it. When the jar is charged to the usual high intensity, I will operate in the capacity of discharging-rod, by presenting the brass ball which I hold in my hand towards the ball of the jar. The result is the simultaneous discharge of the six pieces of ordnance, but not the slightest shock is experienced by myself, although the electric fluid that discharged the guns traversed my arms and chest, prior to its arriving at them.

Glass tubes, filled with water, have been employed in place of the wet string, in the process of firing gunpowder by electric discharges; but if the tube be too wide, the electro-momentum would be too great for any person to stand in the circuit without experiencing a severe blow; and even by long use of a narrow tube the electric momentum becomes too great for any person to stand in its way. I have employed a glass tube of narrow bore for several successive years, but I found that it gradually augmented the velocity of the transmitted electric fluid, till eventually I could not ignite gunpowder by its use: unless, indeed, I considerably augmented the quantity of fluid transmitted. This circumstance, I afterwards found, was owing to the nitric acid that had been formed by previous discharges, from the atmospheric air which the water held in solution, thus increasing its conduction, and giving greater freedom to the electric transmission.

This fact, which I have not seen mentioned in any other place, would not happen if a new portion of water were employed in every experiment, and the inside of the tube well cleaned. Another fact that I sometimes meet with, when operating on gunpowder, is that of setting the string on fire, at one or both of its ends, when they happen to get too dry by previous discharges.

Shortly after the discovery of electro-magnetism was made known in England, Sir Humphrey Davy magnetized steel needles by electrical discharges from a battery of Leyden jars; as will be seen by a reference to his excellent paper in these *Annals*.* But I am not aware that those experiments were ever varied by a change in the character of the circuit, prior to those which I published in 1826; in the *Phil. Magazine*. By those experiments I showed that, unless the electric fluid has a considerable momentum, steel needles are not magnetized by its influence whilst traversing a circuit from the Leyden jar.†

* Vol. vii. † See the first and second papers in No. 47, of these *Annals*.

EDITORIAL NOTICES.

We have heard nothing further respecting the enquiries of T. H. than that our letter, a copy of which appears at page 221, Vol. 6, had been received. The Editor of the *Nautical Magazine*, merely acknowledged it on the cover of his No. for April, 1841.

The "Reviews" which Mr. B. thinks necessary to be continued, are not lost sight of. We have a long time ago been advised, by one of the first chemical philosophers in the land, to carry them on in regular series. Want of time alone, has prevented us undertaking the arduous task. We are aware of the importance of such an undertaking were it for no other purpose than that of exhibiting, to the writer of those papers, the propriety and importance of rectitude, candour, and consistency. We are of opinion that a series of commentaries, properly conducted, and in the same order that the papers have appeared, would be exceedingly interesting.

Dr. Faraday has refused answering Dr. Hare's second letter, *Annals*, Vol. 7, p. 351, and declined any further public discussion with him on the points at issue.—*Silliman's American Journal*.

Prize Volumes for the latter half year of 1842.

1st. For the paper descriptive of a more powerful and economical voltaic battery than any, at present, known. The test to be, *decomposition of acidulated water*, with the terminal metals at one inch asunder.

2nd. For another voltaic battery; conditions as before. Test, *the decomposition of pure water*, with terminals four inches asunder.

3rd. For a thermo-electric battery, superior to any now known, differently constructed, and more economical. Test, *the ignition of the greatest length of platinum wire, of $\frac{1}{100}$ th of an inch diameter*.

4th. For any magnet that will suspend 1 cwt., with the cross-piece at one inch distance from the poles.

END OF VOLUME EIGHT.

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ERRATA.

Page 22,	line 6,	for "as 2·4 15·35,"	read "as 2·4 to 15·25."
— 23,	— 17,	<i>dele</i> the last r in	"heterogeneity."
— 26,	— 17,	for "multiplies"	read "multiplier."
— 80,	— 28,	— "crusis,"	"crucis."
— 81,	— 5,	— do.	do.
— 84,	— 41,	— "fig. 1,"	"fig. 4."
— 87,	— 10,	— "fig. 1, plate 4,"	read "fig. 6, plate 3."
— 174,	— 21,	— "uninsulating,"	"uninsulated."
— 251,	— 10,	— "finished,"	"furnished."
— 251,	— 18,	— "discharging,"	"charging."
— 251,	— 29,	<i>dele</i> "and."	
— 330,	— 36,	for "are,"	read "is."

OMISSION.—The following note ought to have been inserted at the bottom of page 145 :—

"Mr. Joule's electro-magnet appears not to be known to Professor Pfaff."—*Poggendorff*.

DIRECTIONS TO THE PLATES.

The four plates headed "Annals of Electricity," are respectively illustrative of Mr. Griffin's paper, page 1; Mr. Joule's paper, page 302; Mr. Sturgeon's papers, page 225; and Mr. Joule's paper, page 219. The binder ought to place these four plates together, in the order of their numbers.

The six magnetic maps belonging to Messrs. Lovering and Bond's excellent paper, happen to be numbered as they appeared in the original; but they cannot be mistaken, and will, therefore, cause no confusion. They ought not to be separated in the binding. The other two plates, for which the Editor has to acknowledge his obligations to the Council of the "Society of Arts," for their kindness in permitting the impressions to be taken from the original copper plates, necessarily bear the numbers of those plates. The heading is sufficient to guide both the binder and the reader.

LEICESTER, PRINTER, MANCHESTER.

fig 1



fig 2

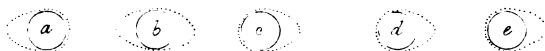


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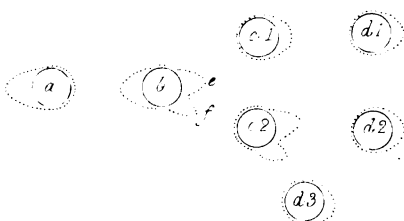


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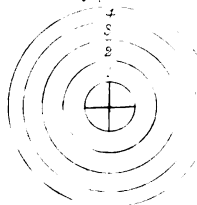


fig 5



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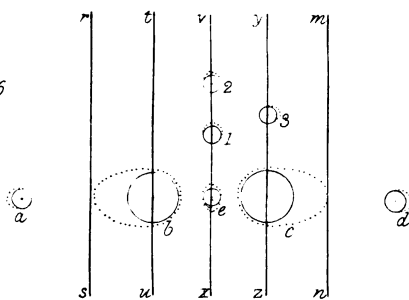


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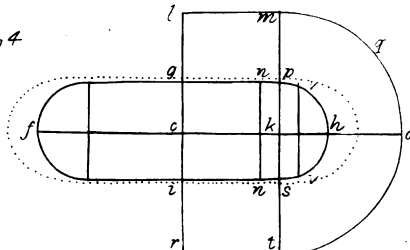
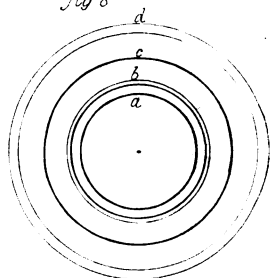
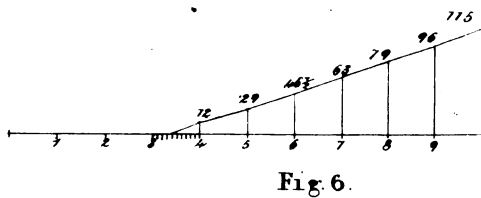
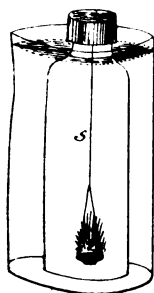
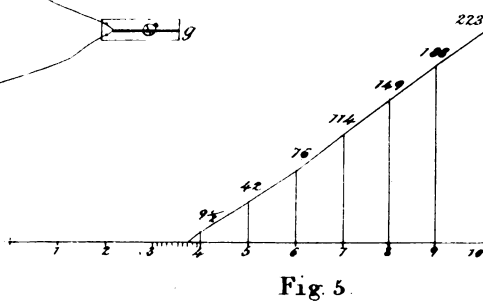
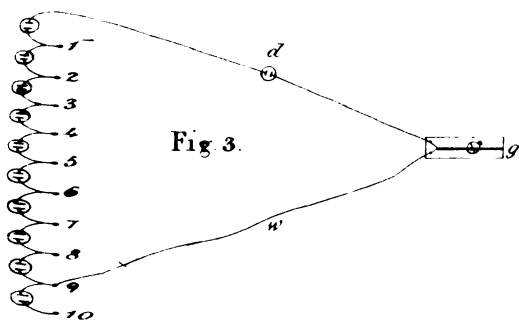
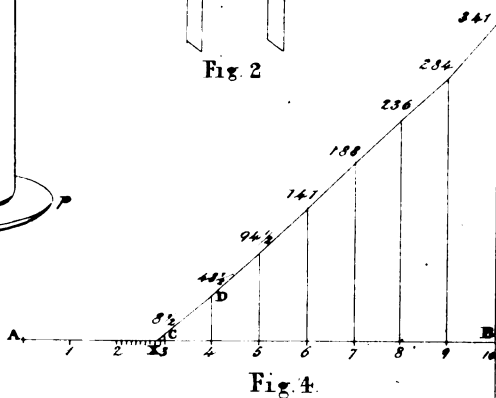
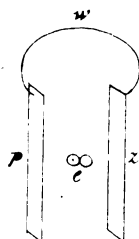
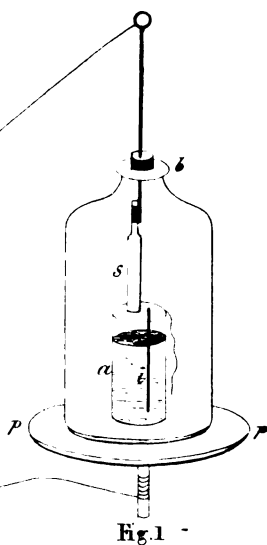


fig 8





J. Forthgill. Lith.

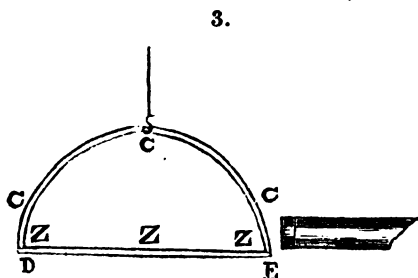
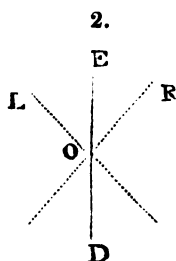
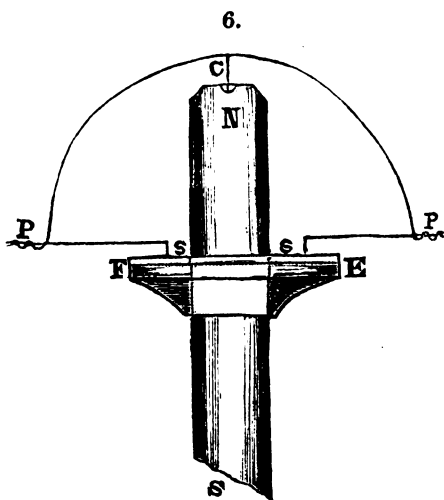
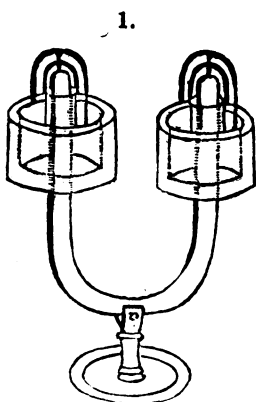
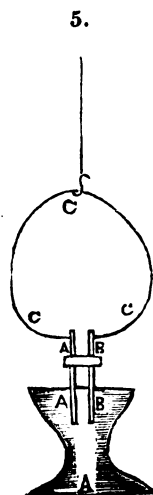
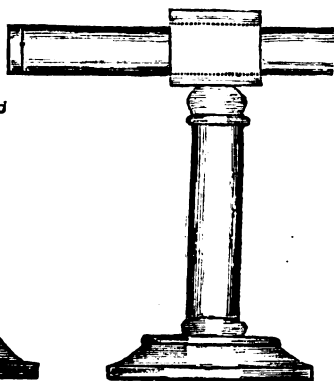
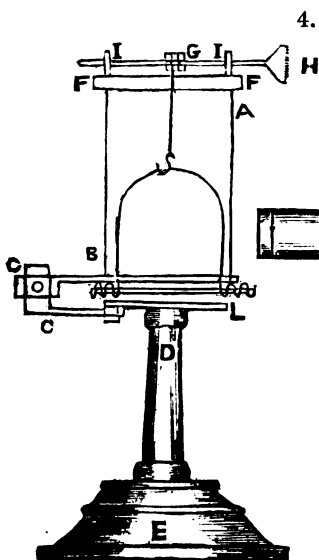


Fig. 1.

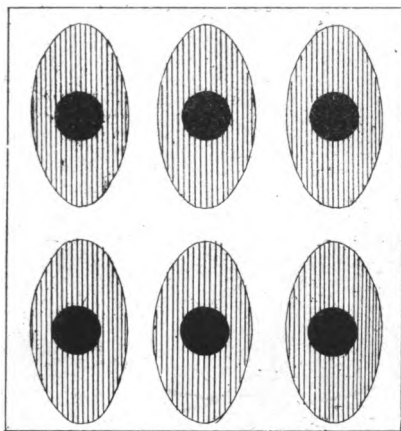


Fig. 2.

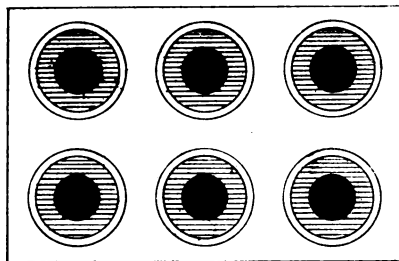
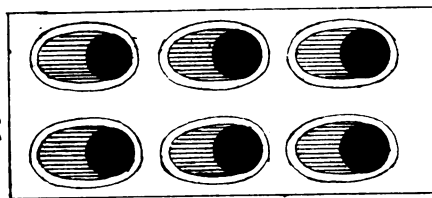
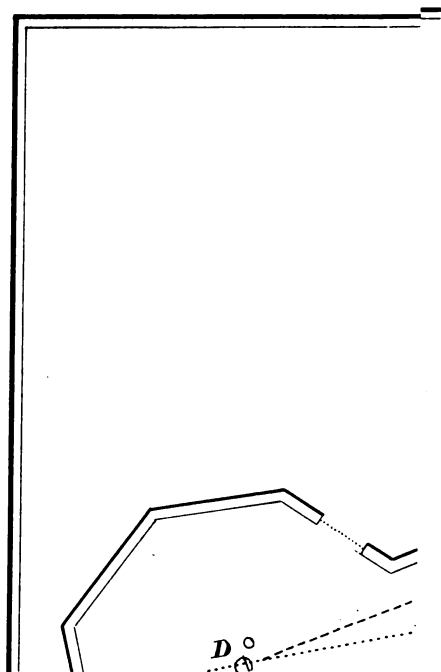
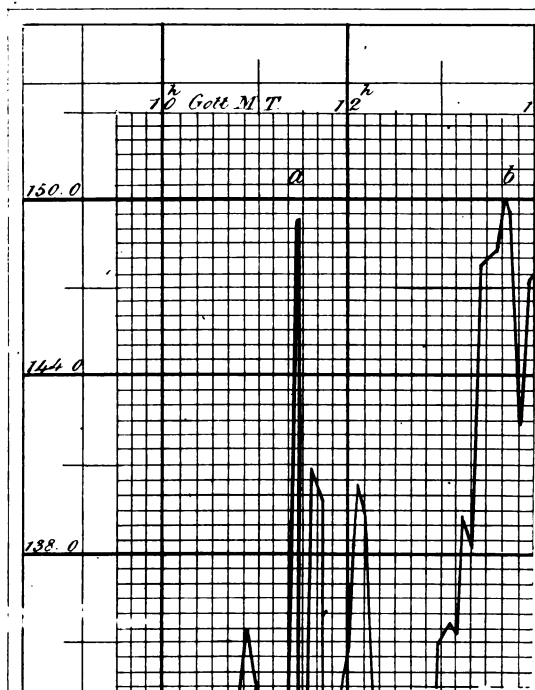
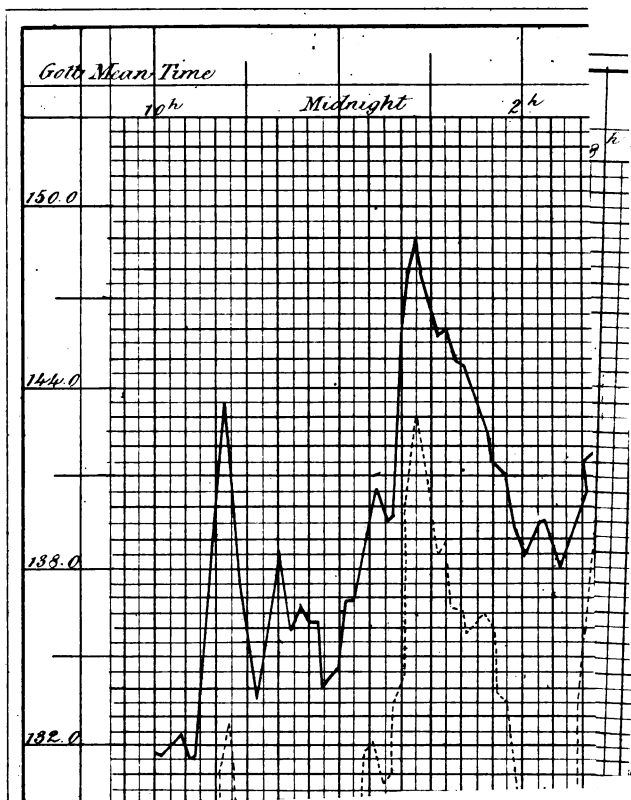


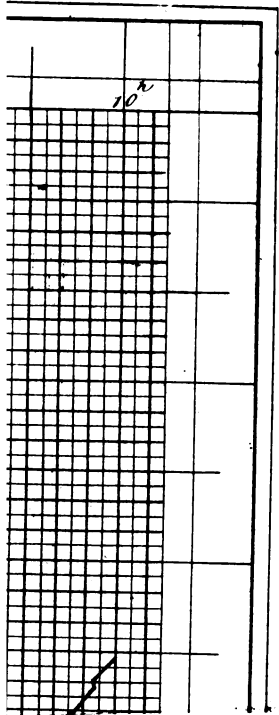
Fig. 3.











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